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FINDING THE WAY AT SEA.

By Richard A. Proctor.

I PROPOSE to give a brief sketch of the methods in use for finding the way at sea, in order that the general principles on which safety depends may be recognised by the general reader.

It is known, of course, to everyone, that a ship's course and rate of sailing are carefully noted throughout her voyage. Every change of her course is taken account of, as well as every change in her rate of advance, whether under sail or steam or both combined. If all this could be quite accurately managed, the position of the ship at any hour could be known, because it would be easy to mark down on a chart the successive stages of her journey, from the moment when she left port. But a variety of circumstances render this impossible.

To begin with, the *exact* course of a ship cannot be known, because there is only the ship's compass to determine her course by, and a ship's compass is not an instrument affording perfectly accurate indications. Let anyone on a sea voyage observe the compass for a short time, being careful not to break the good old rule which forbids speech to the "man at the wheel," and he will presently become aware of the fact that the ship is not kept rigidly to one course even for a short time. The steersman keeps her as near as he can to a particular course, but she is continually deviating, now a little on one side now a little on the other of the intended direction; and even the general accuracy with which that course is followed is a matter of estimation, and depends on the skill of the individual steersman. Looking at the compass card in steady weather, a course may seem very closely followed; perhaps the needle's end may not be a hundredth part of an inch (on the average) from the position it should have. But a hundredth part of an inch on the circumference of the compass card would correspond to a considerable deviation in the course of a run of twenty or thirty knots; and there is nothing to prevent the errors so arising from accumulating in a long journey until a ship might be thirty or forty miles from her estimated place. To this may be added the circumstance that the direction of the needle is different in different parts of the earth. In some places it points to the east of the north, in others to the west. And although the actual "variation of the compass," as this peculiarity is called, is known in a general way for all parts of the earth, yet such knowledge has no claim to actual exactness. There is, also, an important danger, as recent instances have shown, in the possible change of the position of the ship's compass on account of iron in her cargo.

But a far more important cause of error, in determinations merely depending on the log-book, is that arising from uncertainty as to the ship's rate of progress. The log-line gives only a rough idea of the ship's rate at the time when the log is cast;* and, of course, a ship's rate does not remain constant, even when she is under steam alone. Then, again, currents carry the ship along sometimes with considerable rapidity; and the log-line affords no indication of their action: while no reliance can be placed on the estimated rates even of known currents. Thus the distance made on any course may differ considerably from the estimated distance; and when several days' sailing are dealt with, an error of large amount may readily accumulate.

For these and other reasons, a ship's captain places little reliance on what is called "the day's work," that is, the change in the ship's position from noon to noon as estimated from the compass courses entered in the log-book, and the distances supposed to be run on these courses. It is absolutely essential that such estimates should be carefully made, because under unfavourable conditions of weather there may be no other means of guessing at the ship's position. But the only really reliable way of determining a ship's place is by astronomical observations. It is on this account that the almanac published by the Admiralty, in which the position and apparent motions of the celestial bodies are indicated four or five years in advance, is called, *par excellence*, the "Nautical Almanac." The astronomer in his fixed observatory finds this almanac essential to the prosecution of his observations; the student of theoretical astronomy has continual occasion to refer to it; but to the sea-captain the "Nautical Almanac" has a far more important use. The lives of sailors and passengers are dependent upon its accuracy. It is, again, chiefly for the sailor that our great nautical observatories have been erected and that our Astronomer-Royal and his officers are engaged. What other work they may do is subsidiary, and as it were incidental. Their chief work is to time this great clock, our earth, and to trace the motions of those celestial indices which afford our fundamental time-measures, in such a way as to ensure, so far as possible, the safety of our navy, national and mercantile.†

* The log is a flat piece of wood of quadrantal shape, so loaded at the rim as to float with the point (that is, the centre of the quadrant) uppermost. To this a line about three hundred yards long is fastened. The log is thrown overboard and comes almost immediately to rest on the surface of the sea, the line being suffered to run freely out. Marks on the log-line divide it into equal spaces, called *knots*, of known length; and by observing how many of these run out, while the sand in a half-minute hour-glass is running, the ship's rate of motion is inferred. The whole process is necessarily rough, since the line cannot even be tautened.

† This consideration has been altogether lost sight of in certain propositions for extending Government aid to astronomical inquiries of another sort. It may be a most desirable thing that Government should find means for inquiring into the physical condition of sun and moon, planets and comets, stars and all the various orders of star-clusters. But if such matters are to be studied at Government expense, it should be understood that the inquiry is undertaken with the sole purpose of advancing our knowledge of these interesting subjects, and the work should not be brought into comparison

Let us see how this is brought about, not indeed inquiring into the processes by which at the Greenwich Observatory the elements of safety are obtained, but considering the method by which a seaman makes use of those elements.

In the measures heretofore considered, the captain of a ship in reality relies on terrestrial measurements. He reasons that, having been on such and such a day in a given place, and having in the interval sailed so many miles in such and such directions, he must at the moment be in such and such a place. This is called navigation. In the processes next to be considered, which constitute a part of the science of nautical astronomy, the seaman trusts to celestial observations independent of all terrestrial measurements.

The points to be determined by the voyager are his latitude and longitude. The latitude is the distance north or south of the equator, and is measured always from the equator in degrees, the distance from equator to pole being divided into ninety equal parts, each of which is a degree.* The longitude is the distance east or west of Greenwich (in English usage, and now most other nations employ the same starting-point for measuring longitudes from). Longitude is not measured in miles, but in degrees. The way of measuring is not very readily explained without a globe, but may be thus indicated:—Suppose a circle to run completely round the earth, through Greenwich and both the poles; now if this circle be supposed free to turn upon the polar axis, or on the poles as pivots, and the half which crosses Greenwich be carried (the nearest way round) till it crosses some other station, then the arc through which it is carried is called the longitude of the station, and the longitude is easterly or westerly according as this half-circle has to be shifted towards the east or west. A complete half-turn is 180 degrees, and by taking such a half-turn either eastwardly or westwardly, the whole surface of the earth is included. Points which are 180 degrees east of Greenwich are thus also 180 degrees west of Greenwich.

So much is premised in the way of explanation to make the present paper complete; but ten minutes' inspection of an ordinary terrestrial globe will show the true meaning of latitude and longitude more clearly (to those who happen to have forgotten what they learned at school on these points) than any verbal description.

Now it is sufficiently easy for a sea-captain in fine weather to determine his latitude. For places in different latitudes have different celestial scenery, if one may so describe the aspect of the stellar heavens by night and the apparent path of the sun by day. The height of the pole-star above the horizon, for instance, at once indicates the latitude very closely, and would indicate the latitude exactly if the pole-star were exactly at the pole instead of being merely close to it. But the height of any known star when due south also gives the latitude. For at every place in a given latitude, a star rises to a given greatest height when due south; if we travel farther south the star will be higher when due south; if we travel farther north it will be lower; and thus its observed height shows just how far north of the equator any northerly station is; while if the traveller is in the southern hemisphere corresponding observations show how far to the south of the equator he is.

But commonly the seaman trusts to observation of the sun to give him his latitude. The observation is made at noon, when the sun is highest above the horizon. The actual height is determined by means of the instrument called the sextant. This instrument need not be here described; but thus much may be mentioned to explain that process of taking the sun's meridian altitude which no doubt every one has witnessed who has taken a long sea-journey. The sextant is so devised that the observer can see two objects at once, one directly and the other after reflection of its light; and the amount by which he has to move a certain bar carrying the reflecting arrangement, in order to bring the two objects into view in the same direction, shows him the real divergence of lines drawn from his eye to the two objects. To take the sun's altitude then with this instrument, the observer takes the sun as one object and the horizon directly below the sun as the other: he brings them into view together, and then looking at the sextant to see how much he has had to move the swinging arm which carries the reflecting glasses, he learns how high the sun is. This being done at noon, with proper arrangements to ensure that the greatest height then reached by the sun is observed, at once indicates the latitude of the observer. Suppose, for example, he finds the sun to be forty degrees above the horizon, and the "Nautical Almanac" tells him that at the time the sun is ten degrees north of the celestial equator, then he knows that the celestial equator is thirty degrees above the southern horizon. The pole of the heavens is therefore sixty degrees above the northern horizon, and the voyager is in sixty degrees north latitude. Of course, in all ordinary cases the number of degrees is not exact, as I have here for simplicity supposed, and there are some niceties of observation which would have to be taken into account in real work. But the principle of the method is sufficiently indicated by what has been said, and no useful purpose could be served by considering minutiæ.

(To be continued.)

with the utilitarian labours for which our national Observatory was founded.

* Throughout this explanation all minuter details are neglected. In reality, in consequence of the flattening of the earth's globe, the degrees of latitude are not equal, being larger the farther we go from the equator. Moreover, strictly speaking, it is incorrect to speak of distances being divided into degrees, or to say that a degree of latitude or longitude contains so many miles; yet it is so exceedingly inconvenient to employ any other way of speaking in popular description that I trust astronomers or mathematicians who may read this article will forgive the solecism.

THOUGHT AND LANGUAGE.

By Ada S. Ballin.

XV.

OTHER cases of great interest are those in which, although the use of language is not wholly lost, the power of co-ordinating words or syllables is greatly impaired. Patients of this class sometimes pronounce words clearly, but misplace them, so that now and then, if the words are written down, they may be fitted together as in a puzzle, and the patient's idea understood. A case of this kind is mentioned by Winslow.* Dr. Bastian† considers that, "roughly speaking, inability to recall names, or the miscalling of persons, places, or things, would be defects going with injuries to, or altered states of, perceptive centres, and might exist with comparatively slight impairment of intellectual activity; whilst, on the other hand, the extreme forms of amnesia, in which wholly

* "Paralysis from Brain Disease." 1875. p. 189.
† "Brain as an Organ of Mind," pp. 637–8.

irrelevant propositions, or a mere jumble of words are uttered, are more likely to be associated with marked impairment of intellectual power—to be dependent, in short, upon injuries or altered states of parts of the brain more specially concerned with such modes of activity."

Winslow mentions the case of a man who, after an attack of paralysis, transposed the letters of words, saying *tufle* for *flute*, *puc* for *cup*, and *gum* for *mug*. Some letters may always be substituted for others; and the same with syllables. One of Trousseau's cases of aphasia during recovery uttered certain monosyllables to which he always tacked on *tif*; in words of two syllables he pronounced only the first, adding *tif* in the place of the second, as *montif* for *monsieur*, *bontif* for *bonjour*.* Mistakes in the pronunciation and use of words and letters occur occasionally in quite healthy persons, and may or may not be recognised by the speaker. When fatigued or inattentive to what I am saying, I have frequently made slips of the kind, such as asking for salt when I wanted sugar—a request to the absurdity of which I was made alive by a laugh going round the table.

The other morning I was attentively reading the newspaper when my brother made some comment on a daring case of robbery, to which I oracularly replied, "When the cheese is stolen, shut the chamber door!" A remark which, as may be imagined, was greeted with some merriment. In cases like this the fault may be found in lack of attention to the words spoken. Attention implies volition, and when the will is engaged in regulating one set of ideas it is not always immediately at liberty to turn to the consideration of another. Thus during what is called "a brown study," or a fit of abstraction, the thinker may be spoken to, and may not reply for some minutes, then he will answer as if he had only just heard the remark. The attention was retained on the first set of ideas until a natural break came in that train of thought, when it was turned to the other train started by the speaker; on the other hand, a reply may be given at random, as above, without the attention being distracted from the original train of ideas.

The exhaustion of fatigue is proportionately great according as the will is more or less exercised, and this, I think, accounts for the fact that words are misused more frequently during fatigue, for it is impossible to maintain attention beyond a certain point of nervous exhaustion; beyond this point sleep, unconsciousness, or delirium supervenes. Suspension of volition is the essential part of sleep, during which an accumulation of nervous force takes place. Hence, during deep sleep, impressions may be made on the senses without being communicated to the mind—*i.e.*, without being attended to; but when the nervous exhaustion has been repaired, similar impressions will at the same time arouse the attention and the sleeper.

During the suspension of attention in sleep, words may be spoken at random and questions answered with more or less meaning, just as when the attention is otherwise engaged in waking moments.

A similar but more complete suspension of the will takes place during the effects of narcotics or anæsthetics. A typical case of this nature occurred recently in the practice of my friend, Mr. R. Fitzroy Benham. Before the performance of a serious operation an anæsthetic was given to the patient, and while she inhaled it she was told to count ten slowly. She had counted as far as seven when she became unconscious. The operation then began, and lasted for nearly an hour. After this the effects of the anæsthetic began to wear off, and the patient was heard to say, "Eight, nine, ten." She then looked up for further instructions, never suspecting that her counting had for a moment been interrupted. In like manner, volition, and with it attention, may be paralysed by brain injury, and in cases of sudden wounds: for instance, when consciousness is suspended by some portion of the skull pressing on the brain beneath, the train of thought may be broken for weeks or months, but continued immediately on the removal of the pressure on the brain. In cases like this it is probable that impressions are made upon the senses, and are even frequently responded to by the lower nervous centres; but that they take no effect on the higher centres of the brain, owing to the impossibility of attending to them. In a naval battle a sailor was addressed by his superior officer as to the position of a certain vessel. Before he could reply, he was struck in the head by a bullet, and remained insensible for several months, until brought back to England and trephined. Hardly was the operation completed, when the patient uttered these words, "She's to starboard, sir!" in answer to that question put so long ago.

All these and similar cases of suspended attention hang together, the differences between them being only of degree, not of kind. The higher or intellectual centres of the brain being, as it were, paralysed, any action which may take place through the medium of the lower centres is unconscious or automatic.

Other cases apparently originate purely in the loss of memory for words. Such may occur in healthy persons, as people say: "The word is on the tip of my tongue, but I can't get it out."

A case of this kind, brought about by brain disease, is the following, recorded by Dr. Bastian.

Case 8.—The patient became suddenly paralysed in the right side of his body, and speech was almost lost on March 12, 1878, but *sight and hearing were good*. He improved slowly, and his condition on April 2 is thus described:—"He recognises common objects, but cannot name them; repudiates a false name and recognises the real one at once when he hears it. Can never remember his own name till it is suggested to him. On being asked to repeat it (Andrews), after a few trials, which vary each time, he pronounces it 'Anstruthers' or 'Anstrews'; his first name (Thomas) seems to come more readily, and he can often attempt this without prompting. But, either after it has been repeated to him or when he says it spontaneously, he pronounces it 'Towvers.' The letter 'L' is difficult for him to utter; sometimes he pronounces it like a 'D,' and at others like a 'V.' He has been taught to count, *and can fairly pronounce the numerals from one till twelve*; after twelve he is uncertain, the articulation and order becoming rapidly worse. He is conscious when he makes a mistake, but cannot correct himself, and ends in a hopeless muddle. *In reading from a book, the words he pronounces have no relation to the print, either in length or sound. Neither does he seem to understand written characters, as he will not attempt to answer a question written on a slate, though he will at once endeavour to respond when the same question is put to him orally.* He, however, *recognizes numerals from one to nine when written*, and is conscious when they are not placed in regular order. He cannot name any coins, but seems to have some idea of their

* I have met with a somewhat similar case in a little boy six years old, whose speech is defective, but who is, nevertheless, a very bright little fellow. He affixes the syllable *na* (vowel as in Italian) to every word he says, and pronounces only the first syllable of words of more than one syllable. Thus, he says *Edena*, when mentioning the gardener's name, *Ede*; *Betna*, for his cousin *Bessie*; and *gardna* for *garden*.

relative value. He indicated on his fingers that sixpence was worth six pennies, not being able from sight to utter its name." On May 10 he showed by his gestures that he understood a case in the police news which he was found reading, and could *copy his own name* with his left hand; but did not attempt to write any less familiar word from sound, even when he showed he quite understood it.*

The cases alluded to in this and the preceding article are strongly illustrative of the fact that those physical or mental characteristics which are the latest acquired in the life of the race or the individual, those which have had the least time to become engrained in it, are the first to be lost in the process of degeneration. Thus we find that in cases where the use of language is only impaired, it is always the most familiar words which are retained, the others being lost. It is the same in healthy people as in my own case. When the brain is suffering from exhaustion, recently-acquired facts and words are forgotten until the nervous system has been restored by rest. This, too, seems to explain the fact which is generally remarked on with surprise, that old people who have lost all memory for recent events can distinctly recall even trivial things which happened in their youth. In case 1, the patient used the more familiar word "potatoes" for the less familiar "pills." When urged to write, it was found that he could write his address fairly well, and wrote "My dear sir" correctly, but filled the sheet with meaningless writing, only the word "wife" being legible until his signature, which was as clear as ever, so that he was thus able to write the words which occurred most frequently in his correspondence when in health. Similarly, case 8 could not copy any word but his own name, although he apparently understood others; and as in this case, we find that in cases 6 and 7 the words retained were those most frequently used. We find also that in cases where the use of language is wholly lost, as in cases 2, 3, and 4, a perfect command over gestures is retained. In case 4 we find that the patient, although unable to speak himself, understood speech, and replied in gestures, just as the young child understands words long before it can use them, and just as we find some of the lower animals understanding certain words, a subject to which I shall revert hereafter. At the next stage we find case 2, who could neither speak nor understand language; but who was readily made to understand gestures, and himself used gestures which were strikingly clear. On a still lower level stands case 5, who could only very slightly understand words, but understood gestures at once, although she could not always answer questions by gesture, and her own pantomime was lacking in clearness.

It appears to me that cases of the nature of those to which I have referred afford the very strongest circumstantial evidence in favour of the theory that gesture or imitative language was historically prior to verbal language as we know it, and further evidence in support of this will be forthcoming as we advance in our inquiries.

Speaking at Barnsley on Monday week, Mr. Thomas Burt, M.P., advocated in the case of colliery explosions that they should be the subject of special investigation by experienced men, such as was held when a great railway collision occurred. He impressed upon his hearers, who were mainly miners, the great danger of omitting to support the roof with props and stays. Taking the last thirty years, the number of deaths through insufficient timbering was nearly double the fatalities from explosions. Greater supervision was required in regard to timbering.

* "Brain as an Organ of Mind," pp. 642-3.

FIRST STAR LESSONS.

By Richard A. Proctor.

THE constellations included in the twenty-four maps of this series are numbered throughout as follows (the names being omitted on the maps, to clear these as far as possible from all that might render the star-grouping less distinct):—

1. *Ursa Minor*, the *Little Bear* (*α*, the *Pole Star*).
2. *Draco*, the *Dragon* (*α*, *Thuban*)
3. *Cepheus*, *King Cepheus*.
4. *Cassiopeia*, the *Lady in the Chair*.
5. *Perseus*, the *Champion* (*β*, *Algol*, famous variable).
6. *Auriga*, the *Charioteer* (*α*, *Capella*)
7. *Ursa Major*, the *Greater Bear* (*α*, *β*, the *Pointers*).
8. *Canes Venatici*, the *Hunting Dogs* (*α*, *Cor Caroli*).
9. *Coma Berenices*, *Queen Berenice's Hair*.
10. *Boötes*, the *Herdsman* (*α*, *Arcturus*).
11. *Corona Borealis*, the *Northern Crown*.
12. *Serpens*, the *Serpent*.
13. *Hercules*, the *Kneeler*.
14. *Lyra*, the *Lyre* (*α*, *Vega*).
15. *Cygnus*, the *Swan* (*α*, *Arided*; *β*, *Albires*).
16. *Pegasus*, the *Winged Horse*.
17. *Andromeda*, the *Chained Lady*.
18. *Triangula*, the *Triangles*.
19. *Aries*, the *Ram*.
20. *Taurus*, the *Bull* (*α*, *Aldeburan*; *η*, *Alcyone*, chief *Pleiad*).
21. *Gemini*, the *Twins* (*α*, *Castor*; *β*, *Pollux*).
22. *Cancer*, the *Crab* (the cluster is the *Beehive*).
23. *Leo*, the *Lion* (*α*, *Regulus*).
24. *Virgo*, the *Virgin* (*α*, *Spica*).
25. *Libra*, the *Scales*.
26. *Ophiuchus*, the *Serpent Holder*.
27. *Aquila*, the *Eagle* (*α*, *Altair*).
28. *Delphinus*, the *Dolphin*.
29. *Aquarius*, the *Water Carrier*.
30. *Pisces*, the *Fishes*.
31. *Cetus*, the *Sea Monster* (*o*, *Mira*, remarkable variable).
32. *Eridanus*, the *River*.
33. *Orion*, the *Giant Hunter* (*α*, *Betelgeux*; *β*, *Rigel*).
34. *Canis Minor*, the *Lesser Dog* (*α*, *Procyon*).
35. *Hydra*, the *Sea Serpent* (*α*, *Alphard*).
36. *Crater*, the *Cup* (*α*, *Alkes*).
37. *Corvus*, the *Crow*.
38. *Scorpio*, the *Scorpion* (*α*, *Antares*).
39. *Sagittarius*, the *Archer*.
40. *Capricornus*, the *Sea Goat*.
41. *Piscis Australis*, the *Southern Fish* (*α*, *Fomalhaut*).
42. *Lepus*, the *Hare*.
43. *Columba*, the *Dove*.
44. *Canis Major*, the *Greater Dog* (*α*, *Sirius*).
45. *Argo*, the *Ship*.

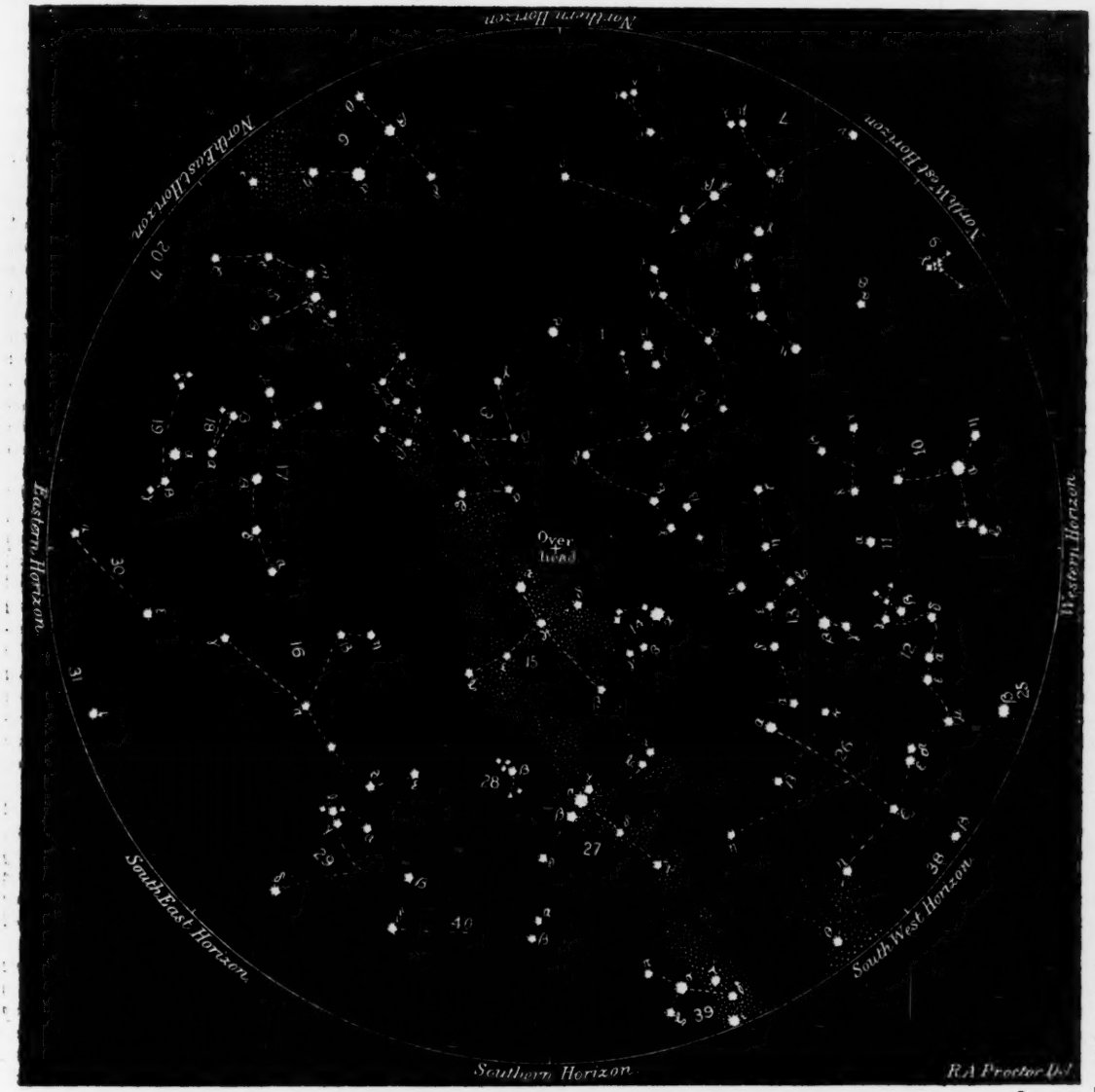
THE PHILOSOPHY OF CLOTHING.

By W. Mattieu Williams.

XV.

I SHOULD add, that only a small percentage of the quilts and other clothing material sold as eider-down are really stuffed with the down of the eider-duck. There are many substitutes—some very inferior, others slightly so. The nearest in quality is the down of the King Duck, an Arctic sea-bird of similar habits to the eider-duck. Goose-down is largely used, the best qualities being obtained from Arctic and marine species.

The necessity for an extra supply of air-holding—not oiled—under down in the clothing of such birds is illustrated by a fact not generally known, which came under my notice some years ago in the course of a cruise in a small schooner from Constantinople to London. We took on board an ample supply of live poultry—fowls and ducks. In the neighbourhood of Malta we had very dirty weather, and Jemmy Ducks failed to shelter the animals in his charge from the wash of the sea; the ducks were miserably wetted to the skin in spite of the supposed oiling of their feathers, they all had cramp, and were killed and eaten "to save their lives." The cocks and hens survived, though in the same quarters. This surprised me, but the captain told me that such is usually the case, and I now find that the faithful follower (referred to in my last) is more easily wetted than barn-



NIGHT SKY FOR AUGUST (Second Map of Pair),

Showing the heavens as they appear at the following hours:—

August 23 at 10 o'clock.	September 3 at 9¼ o'clock.	September 15 at 8¼ o'clock.
August 26 at 9¾ o'clock.	September 7 at 9 o'clock.	September 19 at 8¼ o'clock.
August 30 at 9¼ o'clock.	September 11 at 8¾ o'clock.	September 23 at 8 o'clock.

door fowls if water is projected forcibly upon it, or if rubbed upon the feathers. I attribute this difference to the dusty condition of the clothing of the cocks and hens. Gilbert White, quoting Ray, says (letter 7) "birds of the gallinæ order, as cocks and hens, partridges, and pheasants, &c., are *pulveratrices*—such as dust themselves, using that method of cleansing their feathers and ridding themselves of vermin." My own observations show that some of the dust remains on the surface of the feathers (as may be proved by picking up a fowl and rubbing its breast or other *short* feathers), and that the dust renders these feathers more repellent of projected water-drops. Other surfaces to which, when clean, water adheres are made thus repellent of water-drops by coating them with dust; each particle of dust having its own adherent atmosphere.

Marine birds, especially those exposed to the wash of surf, require a deep and firmly repellent coating of down on their breasts to save them from the fate of our ship ducks. In illustration of the remarkable buoyancy of these, which I attribute to the aërial envelope retained by their abundant down, I may refer my readers to a picture of the red-breasted merganser—an arctic seagoose—in the current number of *Hardwicke's Science-Gossip*, page 181. The quilled feathers are not immersed at all, only the down of the breast.

I am treating this part of the subject rather more fully than originally intended, as I find that the philosophy of

feather clothing is misunderstood to a greater extent than I anticipated. Thus "G. A.," in KNOWLEDGE, Aug. 7, p. 122, still contends that the oil-glands of the duck are used for oiling the feathers by a painting action of the beak, head, and throat. As already stated, I do not question the structure of the uropigium, as described by "G. A.," and have no objection to calling it an oil-gland. I regard it simply as an exaggerated pair of sebaceous glands resembling in secretion and function, though not in form, the sebaceous follicles described in my last, and the smaller and similar glands at the roots of the wing-feathers. I find the "two little heart-shaped glands," &c., in corresponding position near the root of the tails of cocks and hens, which, as Ray says, are *pulveratrices*. They powder their feathers with dust, which would become dirty adhesive paint if the secretion of the heart-shaped glands were used for oiling them. Besides this, I have examined the bill, head, neck, and back of a duck after it has nibbled and rubbed in the manner described by "G. A.," and positively deny that these parts are then or at any other time smeared with any such fat or oil as stated. The supposed paint-brushes are demonstrably less greasy than human skin, as may be seen by rubbing a clean piece of metal or glass upon the feathers of the head and neck of the duck, and a similar piece in like manner on one's own face and neck, and then comparing the smear. The ducks and swans in our London parks supply further proof. The white species remain white, instead of sharing the colour of London sheep, which they undoubtedly would if their feathers were greasy or oily like the wool, and exposed to the carbon flocculi which shower through our London atmosphere. The brushing action of the head and neck is not always preceded by a pinching of the oil-glands, nor usually so, as anybody may learn by watching the ducks in our parks. They clean their feathers, but do not smear them.

Upon this question of whether the water repulsion depends upon oiling of feathers or upon the adhesion of a film of air to them depends the verdict we must pass upon the desirability of using feathers for bedding and for clothing. At one time I joined with high sanitary authorities in denouncing feather-beds on the ground that, being organic animal matter, they should be liable to organic decomposition, and to saturation with condensed cutaneous exhalations. It is not very long ago since I wrote to my friend Dr. Richardson on the subject; but further investigation now induces me to recant, and the grounds of this recantation are the same as my repudiation of the supposed feather-oiling by ducks.

I find that feather fibres generally (I say "fibres" in order to exclude feather quills) are remarkably free from greasiness or humidity, *more so than any other animal structure* that I am acquainted with. I find also that they are exceptionally free from the vice of condensing sebaceous and other skin exhalations on their surface; that while cotton, linen, woollen, and silken fabrics become defiled by perspiration, feathers, similarly exposed, remain clean. The contents of a flock-bed after a few years of continuous use become matted and foul, while those of a goose-down bed after a similar or much longer period remain untainted; they are only dusty, the dust chiefly due to the fracture of feather filaments. I have examined feathers from a bed that has been half-a-century in continuous use, and find that, after a slight shake, the feathers are as clean as when they were new. Not so the enveloping fabric either inside or out, although it has been washed and renewed frequently during the lifetime of the feathers.

This, if am not altogether mistaken, depends upon the fact that every filament of the down attaches to itself an obstinately-adherent film of air, which prevents the condensation of moisture on its solid surface. Thus we may lie on a feather-bed in a hollow formed by the pressure of our bodies, may perspire freely and moisten the coverings of the feathers, and yet the feathers themselves shall remain dry and untainted, the vapour diffusing itself throughout their adherent atmospheres. The advantage of this is obvious.

Down skirts are made and used, and must be far better than ordinary skirts of similar form, as the required amount of protection is obtained with far less weight. Whenever the merciless despotism of fashion may command its slavish victims to render themselves hideous by exaggerated nether expansions, either behind, or in front, or all round, the unfortunate creatures will suffer less by using light eider-down enlargements rather than metal cages or other heavy substrata. Let me not be misunderstood; I am not recommending such things on their own merits, but only as lesser evils in the meantime, until we can afford to erect lunatic asylums of sufficient capacity to stamp out the fundamental evil.

Feathers have been used as clothing materials by the most primitive of savages, usually by attaching the quill ends of the feathers to each other, and thus building up a mosaic fabric of highly ornamental as well as useful character. That from the South Sea Islands, exhibited by Lady Brassey at South Kensington last year, is a fine example, and said to be of great value. The labour expended on every square yard of such material must be immense; but whether it may be substituted by machinery I am unable to say. Another and a better mode of availing ourselves of the valuable clothing properties of feathers suggests itself, viz., that of carding and weaving the filaments of down, such as common duck and goose down, swans-down, &c. This appears to me to be quite a soluble problem, and I recommend it accordingly to the attention of ingenious mechanicians, believing firmly that such a fabric would, for ordinary clothing purposes, excel all others in lightness, warmth, and durability, with the additional excellence of allowing free passage of the perspiration and being waterproof without any oiling.

Vests and shirts of woven down would, I have no doubt, attain the nearest possible approach to perfection in underclothing for both winter and summer. They may, probably, be difficult to clean. Ordinary scrubbing with soap and water and soda is not likely to succeed. The laundress of the future in dealing with these will have to take lessons from the birds, probably from the *pulveratrices* above-named, and use cleansing powders applied with the aid of suitable brushes—feather brushes, like the duck's head.

OUR HOUSEHOLD INSECTS.

By E. A. Butler.

COLEOPTERA (*continued*).

ONE of the finest, though at the same time most destructive, divisions of the beetle order is that called Longicornia, or Longhorns. The beetles are many of them remarkably handsome and of considerable size, and are readily distinguished by the great length of their antennæ, which, in some cases, many times exceed even that of the body itself. These insects, in their larval condition, burrow into the solid wood of timber trees, where they live, often from three to five years, devouring

the heart wood and utterly ruining the timber by excavating through it in various directions neatly-cut galleries, which, commencing on the outside in a small and scarcely noticeable opening, constantly increase in diameter with the growth of the larva.

As a consequence of their longevity and the seclusion of their life, it not unfrequently happens that when an affected tree is cut down and has been sawn up into planks, the latter contain some of the immature larvæ, which escape notice through their burrows not having been sawn through, and thus get conveyed into timber-yards, and even used in building construction, before their occupants have had time to complete the necessary arrangements for making their *début* in beetle society. After awhile, however, this important era in the life of the insects arrives, and the beetles make their exit from their burrows, only, however, to find themselves far away from their native forests, strangers in a strange land, and suddenly introduced into a human society, which is as astonished to receive them as they are to find themselves in its presence. In this way many fine exotic Longicorns have been captured alive in different parts of England, and this, too, is the explanation of the not unfrequent occurrence of the Longicorn beetle called the "Timberman" in mines; they have been introduced, in the larval condition, in the timbers used in roofing and supporting the passages, and have sometimes established themselves and bred there. Various forest trees are liable to the attacks of Longicorn beetles; but, of course, it is those that burrow in fir-wood that are chiefly imported into this country.

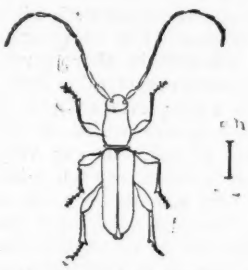


Fig. 1.—Gracilia pygmæa.

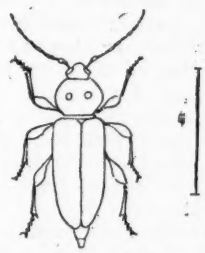


Fig. 2.—Hylotrupes bajulus.

So, then, there are some of these Longicorns that may every now and then be expected to turn-up in houses. Our British species are few in number, and, as a rule, not common; still, I have received one of our smallest in considerable numbers from two different houses. It is a quaint little brown beetle, which is said to be partial to old woodwork, and is called *Gracilia pygmæa* (Fig. 1).

It is a narrow, linear insect, with antennæ only a little longer than the body, the length being produced, as in all this section, not by a multiplication of the joints, but by their individual elongation. It is remarkable for the disproportionate size of the thorax, which, with the head, occupies about one-third the length of the whole body, and for the great breadth of the thighs at their outer extremity. The antennæ, as might be expected, are very liable to damage, and as the insects are pugnacious, if several of them are confined together they are sure to fight, and as a consequence a great mutilation of antennæ and legs ensues, the battle-field being strewn with the fragments.

It has also been found, in large numbers, burrowing in the twigs of a hamper, which, small though they are, afford plenty of scope for our pigmy beetle. Baskets form, in one way or other, an easy means of transference for insects from one country to another. Many continental species are brought over with fruit and vegetables, and the Borough Market, in London, is quite noted for the number of such insects that have been found alive there. About five years ago a French longicorn was introduced in large numbers by means of a basket; some escaped and were afterwards found out in the open, when it seemed as though a new British beetle had been discovered. Fortunately, however, their captor was a coleopterist of repute, and he, by means of careful observation and inquiry, managed to elucidate their history. The account is best given in his own words:—"During the July of 1880, one of my servants brought me two specimens taken in the garden at the back of the house (the only two specimens then noticed). Last July, however (1881), two or three more were captured, and a day or two after they called my attention to the fact that numbers (dozens, in fact) were creeping upon the floor in the scullery: upon examination, I traced them to an old basket used for potatoes, and generally kept under the slopstone, and consequently moderately damp; in this they showed their presence by numerous small round holes, about the size of a pin's head." The basket, on being submitted to a professional basket-maker, was pronounced to be "of French make from Dutch willows." They had, therefore, evidently established themselves in the basket while in their native country, and subsequently accompanied it across the Channel, when it was used for the transport of vegetables.

The larva of a much larger beetle, called *Hylotrupes bajulus* (Fig. 2), has sometimes done considerable damage to the rafters of houses, not only perforating the wood, but even gnawing its way through sheets of lead with which the rafters were covered. Kirby states that Sir Joseph Banks once gave him a specimen of sheet-lead, which, though only measuring eight inches by four, was pierced with twelve oval holes, some of which were as much as ¼-inch in longest diameter. The generic name *Hylotrupes*, which is Greek for a "borer of timber," at once stigmatises the insect as destructive in this way.

The beetle is a blackish insect covered with greyish down, and the name *bajulus*, which is Latin for a "labourer," is apparently given in allusion to the dusty appearance caused by this down. The antennæ are of no more than ordinary length, so that at first sight it would perhaps hardly be taken for a Longicorn at all. The thorax is very globose, and carries two polished knobs on its upper surface. The thighs, like those of *Gracilia*, are clubbed, only more conspicuously so.

The larvæ of these beetles are fat, white, fleshy grubs, with small, but very powerful, black jaws—the tools by which all the damage is effected. The pupa is formed in the burrows.

Fir palings in gardens sometimes produce plentiful supplies of a most lovely beetle, the resplendence of whose appearance is such as to suggest, though falsely, an acquaintance with the glowing rays of a tropical sun, instead of the comparatively feeble beams with which Old England is favoured. It is entirely of a most lovely violet or deep blue colour, and is shaped not unlike *Hylotrupes*, though flatter, and with longer antennæ. In allusion to its colour, it is called *Callidium violaceum*.

Here is a marvel in physiological chemistry! The larva is absolutely white, except for its little black jaws; there is not a trace of blue or any other colour about it, even up to the very time when it ceases feeding and changes into a chrysalis; and yet during its larval existence has been stored up in its body something from which, by the changes that take place during the pupal

stage—a period during which no additional nutriment is taken—is elaborated the gorgeous hue which glorifies its adult form.

Longicorns are frequently very variable in size, as is usually the case with wood-boring insects. Imprisoned as they are in a burrow which, as larvæ, they never leave, they have very little power of selection of food, and are, therefore, entirely dependent upon the supply into the midst of which their excavating labours carry them, and according to the quality of this will be the vigour, or otherwise, of their constitution, and the stature to which they will attain.

(*To be continued.*)

THE GREAT RED SPOT ON JUPITER.

By Richard A. Proctor.

(*Continued from p.* 106.)

THE spot was manifestly a surface feature, in this sense that the layer of clouds in which the spot appeared as a sort of opening was part of the visible surface of the planet. This was shown by the circumstance that as the spot drew near to and passed the edge of the planet its outline remained distinctly visible. Had the spot been due to some formation lying below the surface clouds of the planet, the spot could not have been thus traced up to the planet's edge. Of course this need not prevent us from recognising the true cause of the spot as existing far below the surface-level; but manifestly the cloud layer was laid open at its outer surface. Now this being so, it is clear that were the forces which formed the spot all at work at that same surface level, and all acting from or towards a centre, we should expect them all to act with about the same degree of force, and the spot to have therefore a circular shape, unless we can recognise some likelihood that in different latitudes on Jupiter different conditions would exist, or in other words unless we can recognise the existence of zones on the planet akin in some sense to the trade and counter-trade wind zones on the earth.

But although the most characteristic feature of Jupiter is the existence, almost always (if not always), of parallel bands or zones of clouds, diverse in their light-reflecting qualities, these zones have no permanent position like the trade zones on the earth. They vary almost capriciously in position. Sometimes there are but four or five of them, at others there are ten or twelve or even more. We cannot recognise any permanent difference, then, in the condition of the various latitudes on Jupiter, to account for the oval figure maintained so long (six years at least) by the great spot.

Yet we may still, or rather we must obviously, associate the lengthening of the great opening in a direction parallel to the cloud zones, with the forces to which the existence of those clouds—as such—is due. Now it has always seemed to me that as the trade wind theory, once complacently advanced to explain the parallel belts of Jupiter and Saturn, most manifestly fails, we are driven to another interpretation of the cloud belts which is very significant in regard to Jupiter's condition. The trade winds and counter trade winds, and the zones named after them, owe their existence to the difference between the rotation velocities in tropical regions which lie farther from the earth's axis and in temperate and arctic regions which lie nearer to that axis. The cloud-belts of Jupiter and Saturn must also be due to differences of rotational velocity,—not however between places in different latitudes on those planets, but between regions at different elevations in the cloud envelopes of Jupiter and Saturn. We seem forced to admit, seeing that the belts are real, and no other way of accounting for their existence seems open to us, that there must be great movements of ascent and descent, in the cloud-laden envelopes surrounding the giant planets. Matter carried upwards, as columns of ascending vapours, or missiles ejected to enormous heights from Jovian volcanoes, passing as such matter does from regions near the centre, where the motion of rotation is slower, to regions higher up, where the motion of rotation is more rapid, lag behind and cause a trailing of cloud forms towards the west. On the contrary, matter descending, as torrents of falling rain, or matter falling back after ejection, would rush forwards and cause the cloud forms to be extended towards the east. Granted a sufficient range in height, whether in ascent or descent, and the parallelism thus arising would be as marked as we actually find it in the cloud-belts of the giant planets.

But here certain questions arise which we must dispose of before we consider in this light the lengthening of the great spot. *Can* we imagine that the cloud-laden envelopes surrounding the giant planets have the enormous depth which this explanation would assign to them? The depth essential for this interpretation must bear a measurable proportion to the diameter of the planet. Less than at least a thousand miles (only a fortieth of the planet's diameter) would certainly not suffice; for obviously the rotational velocities at the top and bottom of a cloud region one thousand miles high on Jupiter would not differ by more than about one-fortieth, or about 2½ per cent., whereas the sharp parallelism of the belts indicates quite a considerable difference of velocities. Taking, however, even a depth of one thousand miles for an atmosphere which at its highest part bears clouds such as exist in our very highest cloud-bearing atmospheric strata, say ten miles above the sea-level, we find a very remarkable state of things at a depth of even but a hundred miles below the visible cloud surface of Jupiter, unless we suppose the laws connecting density and pressure to be very different on Jupiter from the laws recognised here,—a supposition which must not unnecessarily be introduced. For our air ten miles above the sea-level has a density equal to about one-eighth the density of the air we breathe, the density doubling for each 3½ miles (or thereabouts) of descent. Taking this density as that existing at the outskirts of the visible cloud-envelope of Jupiter, we find that with his known (and well-measured) gravitating energy, the density would double for each mile and a half of descent. But say that it doubles only once for two miles of descent. Then four miles below the visible surface of the planet the atmospheric density would be already half (instead of one eighth) of our air's at the sea-level; six miles below it would equal the density of our air; eight miles below it would be double; and only ten miles below the visible surface of Jupiter the density of his air would be four times the density of the air we breathe. After that, in the next ninety miles of descent, taking us only one hundred miles from the surface, there would be forty-five doublings of pressure and density, making the density millions of millions of times greater than that of air, thousands of millions of times greater than that of water, and hundreds of millions of times greater than the density of any terrestrial element. This of course is altogether prepos-

terous. But it shows that in one way or another we have to admit the existence of conditions in Jupiter which are utterly different from any known on earth.

(To be continued.)

THE YOUNG ELECTRICIAN.

By W. Slingo.

(Continued from p. 113.)

Ex. C.—A simply-performed experiment is illustrated in Fig. 55. A B is an electrified rod (of any convenient material, in this case glass). G G are dry warm wine-glasses, E E are eggs placed on the glasses as shown, and touching each other, as at F. On A B being brought near C the eggs become inductively electrified, the near end, C, assuming a state (negative) opposite to that on the adjacent electrified rod. The end D of the other egg becomes simultaneously charged with positive electricity, and were means readily available for investigating the condition of either egg at F, that condition would be found to be neutral. With such investigations we may deal presently. If, while the state of affairs remains as indicated in the diagram, the approach of a suspended feather or pith ball to D will result in attraction. Assuming the suspended substance to have been kept from actual contact with the egg, and to be removed together with A B, the eggs will return to the neutral state, neither D nor C showing any signs of electrification. Nothing has been given to or taken from them.

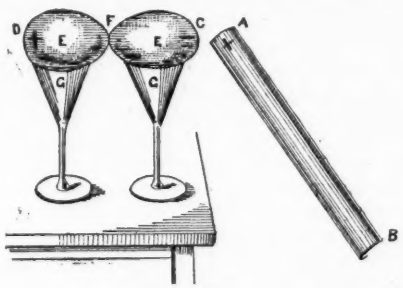


Fig. 55.

Let A B again approach C, the eggs become electrified as before. While A B is still in the vicinity of C, separate the remote or positively-charged egg to a short distance —say half-an-inch—from the other. This can be done with the left hand (catching hold of the glass as low down as possible, and with dry fingers), the right hand still grasping the end, B, of the electrified rod. Next remove A B to a considerable distance. The eggs will now show feeble signs of electrification at C and D, because the opposite charges, in their effort to produce the neutral state, have taken up their position at the adjacent extremities, F. Let the eggs be now separated to a greater distance, so as to be practically out of each other's range of influence. If no other conductor is near the charges on the eggs will be distributed over the surfaces, the greatest accumulation taking place at the ends, because the self-repellant nature of electricity is more nearly satisfied there than at any other (and therefore less distant) parts of the egg. Both eggs will then exhibit more or less powerful charges of electricity, and will be capable of attracting light bodies. After discharging the eggs, let them be again brought into contact and let the positively electrified rod A B again approach C, so as to once more electrify the eggs. Then let D be touched by the finger or any other large conductor; D will be by that means neutralised, and, however great may be the electrification at C, no charge can be maintained at D. The same result would follow were the finger placed at any other part (except that part immediately opposite A) of either of the eggs. In effect, the remote end of the conductor C D is removed to an infinite distance (as indicated in Ex. XCIX.). Let us confine the finger to some portion of the remote egg E D. The removal of A B will allow the negative charge on the other egg to be neutralised. This will not happen if A B be kept near C. Let the finger remain on the remote egg, and let that egg be removed. It will, of course, be neutral; but the subsequent removal of A B will leave the near egg charged with negative electricity, and it will be able to show its charge by any of the previously-mentioned methods.

These two eggs, then, teach us a great deal. Indeed, they may presently teach us a deal more.

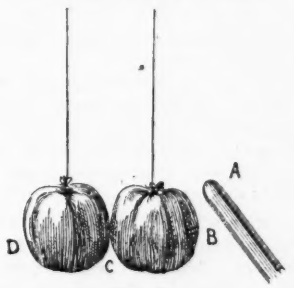


Fig. 56.

Ex. CI.—Fig. 56 illustrates another device for performing all the experiments contained in Ex. C. A is an electrified rod; D C, C B are two apples (with the stalks cut short) suspended by means of dry silk threads from any convenient support, such as a horizontal wooden lath or even a metal rod. The longer the thread, that is, the greater the distance from the horizontal rod, the better. Pared potatoes, turnips, rounded carrots or parsnips, would answer equally well, for all are sufficiently good conductors.

Ex. CII.—A similar experiment, but on a somewhat more elaborate scale, is illustrated in Fig. 57. D and C are cylinders coated with tinfoil or other thin metal, similar in every respect to the one described in Ex. XCVII. S S are the insulating supports, and A B is the

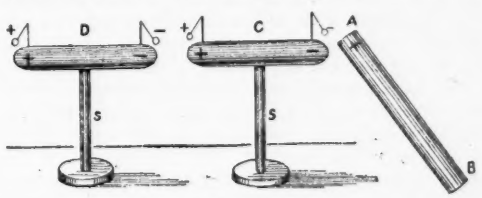


Fig. 57.

rod electrified, let us say, positively. The near end of C becomes negatively electrified, and the remote end positively. We now get a species of induction by deputy. For the positive charge on the distant end of C acts in its turn inductively on the adjacent cylinder D, making the near end negative and the remote end positive. This state of affairs is evidenced by the positions assumed by the suspended but uninsulated pith balls. They mani-

festly share the charges of the parts of the cylinders to which they belong, and equally manifestly, they are therefore repelled by the respective cylinder-ends. The two central balls are not only repelled by the cylinders, but, being oppositely charged, become mutually attractive. If D be touched with the finger, the positive electricity at the distant end will, as in the experiment with the eggs, be neutralised, and the pith ball in proximity will also lose its charge, whence it will fall into the vertical position. Similarly, the finger being withdrawn, and the adjacent ends of the two cylinders brought into contact, will result in a further neutralisation, and the two central balls will also hang vertically. C D will then act as one long cylinder, a greater degree of electrification will probably be produced at the negative end of C, and a positive charge will in consequence appear at the distant end of D equal in quantity to the increase of negative on C. Other experiments with these cylinders may be inferred from the experiments with the eggs.

DUAL GENERATION.

Pr. 5.—The two electric states are always produced together and in equal quantity.

To one conversant with the nature, or rather the behaviour, of electricity, this requires no demonstration. It is well, however, for beginners to have it always in mind. The induction experiments which we have just performed demonstrate the fact pretty clearly, and emphasis was laid upon it in Pr. 1.

Ex. CIII.—Rub a stick of sealing-wax with a piece of flannel or a pad measuring about 5 inches by 4 inches, and composed of five or six layers of flannel sewed together. Let the flannel be insulated from the hand by interposing a piece of sheet gutta-percha (procurable at most of the shops where gutta-percha goods are sold). Either the sealing-wax or the flannel will then be found electrified, although oppositely. That their charge is equal may be proved by presenting both bodies together to a suspended pith ball. The absence of attraction proves the equality; for were one charge greater than the other, that one would exert a preponderating influence and cause attraction.

THE ISLE OF WIGHT STEAM FERRY.*

THE works at Langston Harbour, near Portsmouth, and at Brading, in the Isle of Wight, designed to effect the transport of trains of railway vehicles across the Solent, were completed on the 5th inst., and on the following day the appliances which have more than once been used provisionally during the execution of the works, were pronounced ready for regular traffic. It should be stated that no claim is laid to mechanical novelty—the merit lies in the ingenuity in which commonplace mechanical arrangements have been applied to achieve in a simple way an exceedingly useful purpose. Railways existing on the mainland and in the Isle of Wight, with their lines coming down to the seaside, the problem to be solved was how to transfer simply, cheaply, and expeditiously from the railways to the deck of seagoing vessels, railway vehicles without disturbance of their load. Once upon the deck of the steamer, supposing only she be adapted to the carrying of a heavy deck load, the transport is simple enough—the thing is the loading and unloading, and to be useful this must be accomplished by means more expeditious and less costly than the ordinary method of handling and transhipment. The distance to be traversed between Brading and Langston is between ten and eleven miles, 6⅓ miles are in the open sea, the rest in land-locked harbours, which, on the island, as on the mainland, have been judiciously chosen as the places of arrival and departure. On both sides the process of loading and unloading goes on in perfectly still water, and on both the mechanical arrangements are the same. The amount of structural work on the island was rather less than that necessary on the mainland, owing to somewhat less favourable conditions at Langston. It goes without saying that on both sides there is direct physical connection with the railway systems. At Brading all the railways in the island are communicated with, while at Langston access is obtained to the Hayling Island branch of the Brighton and South-Coast Railway, which at Havant has a junction with the London and South-Western system—to London by the direct Portsmouth line as well as to Southampton and the West of England.

At Langston the railway skirts the sea. Alongside of, and parallel to, the railway and upon the foreshore an embankment has been built about 700 yards in length, and of width varying from 30 to 40 yards. The side or sea-face of this embankment is sloped and pitched in the customary manner for its entire length, save 300 ft. at the extremity, where a wharf is formed suitable to the use for loading and discharging of ordinary sea-going craft. From the end of the embankment a sloped timber jetty projects, commencing at the rail level and descending by a gradient of 1 in 8 to 4 ft. below the natural bed of the sea. From the top to the bottom of this slope are laid eight ordinary permanent-way rails, four of which constitute two running lines of the standard gauge, and along which, as presently explained, the railway vehicles pass, and four laid close, and parallel, to each of the running rails. These latter act as check rails, but fulfil also a more important purpose to be described.

In carrying the waggons on board the steamer, they are placed upon two lines running from stem to stern. In discharging and loading them, the vessel approaches the sloping jetty stern on, bringing the parallel rails upon her deck into line with the rails laid upon the sloping jetty. It follows that when the tide is high and covers the greater part of the slope, the level of the steamer's deck approaches the level of the top of the jetty; when the tide is low, the steamer approaches at a lower level, and a considerable part of the slope is exposed; but neither at high or low tide can the stern of the steamer be brought sufficiently near to the fixed slope to admit of waggons passing from the one to the other. There is always a hiatus which must be bridged. The four extra rails above mentioned are useful in this connection. They carry the moving bridge or cradle, which, passing up and down the sloping jetty in the varying states of the tide, connect the ship, at whatever height her deck, with the rails on shore.

The cradle, which is supported on twenty wheels, resting five on each of the four rails of the jetty, is of timber with wrought-iron attachments and cast-iron wheels, the movable drawbridge being balanced so as to be well within the power of a single man. It is moved up and down the slope by means of drawing engines, which also drive two horizontal drums 3 ft. in diameter, by means of which waggons are lowered on to and drawn up from the deck of the steamer. The drawing engines are a pair of ordinary winding engines of 60-h.p.

* From *Engineering*.

Attached to the drawing machinery is a movable shunting capstan for economising locomotive power in the station-yard. The steamer is of iron and of great strength, her dimensions being—length, 130 ft.; breadth of beam, 26 ft.; h.p., 150 nominal; draught loaded, 5 ft. 9 in. She has the steam steering-gear of the Harrison type.

The deck arrangement is noteworthy, from the position of the lines of rails. What would in an ordinary railway be the 6 ft. space, is 4 ft. 8½ in. It follows, therefore, that in the event of there not being a sufficient number of waggons to occupy the two outer lines of rails, the load may be placed amidships on the central line, and so contribute to the steadiness and trim of the vessel at sea. The process of loading and discharging may be briefly described. On the approach of the ship, with her cargo aboard, the person ashore in charge of the engines and cradle observes the state of the tide, and, knowing the draught of water, adjusts the cradle by lowering it or raising it to the required level. On the vessel coming into position, the drawbridge, which is raised and depressed by crabs worked from the gantry, is lowered on to the steamer's prow and made fast there. Ropes, which are ready on the drawbridge connected with the winding-gear, are then hooked on to the coupling-chains of the foremost waggons, and on signal being given the whole train is drawn out at one operation. The time occupied in unloading is regulated by the speed at which the engines are run, and this may be fast or slow according to the condition of the tide and other circumstances. At high tide, when the deck of the steamer and the cradle are nearly level with the rails at the top of the slope, the process of discharging may last some thirty or forty seconds. At dead low water, when the slope is at its maximum of steepness, a slower speed is advisable, and the time occupied may vary from two to four minutes. The loading, which is accomplished on a similar principle, requires rather more caution. The waggons, being drawn to the verge of the slope by steam shunting-gear attached to the winding engines, are then allowed to run on to the deck by their own gravity, checked and regulated by the ropes attached to the drums.

When the project was first mooted, doubts were freely expressed as to the sufficiency of traffic to warrant an establishment of this kind. Recently, however, doubts on this subject have been resolved, and it is now anticipated that difficulty is more likely to arise from redundancy than deficiency of freight. The present carrying power, judging from the traffic that is already offering, is likely to require augmentation. Already inland coal traffic is tendered for conveyance fully up to the carrying power, and other branches of traffic to which the system lends itself if presented in the quantity that seems probable, can only be accommodated by an additional vessel. As some indication of the need for improvement which the Transit Company supplies, we quote from an official source a brief description of the plan which the new arrangements supersede. Speaking of goods seeking delivery at the Isle of Wight, the writer remarks:—

> At Portsmouth, where the first handling and delay occurs, everything must be unloaded at the town station and take turn with Portsmouth town goods; then follows cartage through the town to the quay, and two more handlings occur here in unloading the carts and shipping.
>
> Arrived at Ryde the goods are moved again for cartage to the station (through the town of Ryde), and once more there in loading into railway waggons.
>
> At Ventnor, or other destination, the reverse process occurs, and after two more handlings and another cartage, the consignee is at last reached, and it is well if he has nothing to complain of in the condition of his goods.
>
> Since arriving at Portsmouth there have been seven separate handlings, three cartages, a risky water passage, and a railway journey. Although the railway company's responsibility continued throughout, their actual control ceased at Portsmouth, when possession was transferred to the Isle of Wight agents or carriers.

In the future, by contrast, vehicles loaded in London will go direct to their railway destination with no more disturbance to bulk or change of vehicle than is involved in a railway journey between London and Birmingham. The whole of the costly and cumbersome terminal services at Portsmouth and Ryde will be avoided.

The plans and local installations are from the design of Mr. Samuel L. Mason, of Edinburgh, who was also the originator of the scheme. He has closely followed the arrangements of the North British Company at Burntisland, of which he had experience when formerly general manager of that company. The work has been financed and constructed by him, the resident engineer being Mr. William Gregory, C.E. Mr. Stroudley, C.E., of the London, Brighton, and South Coast Railway Locomotive Department, constructed and erected the machinery.

It is in great measure due to the enterprise of the Brighton Board, to their manager, Mr. J. P. Knight, goods manager Mr. Stainforth, and to Mr. Spencer Balfour, M.P., that the Isle of Wight is secured the possession of an economic means of communication of great promise and capability. The Brighton Company have, we are informed, entered into agreements by which they adopt the new route for the whole of their traffic, under conditions which give assurances of financial success.

Reviews.

SOME BOOKS ON OUR TABLE.

The Anatomy of the Intestinal Canal and Peritoneum in Man. By Frederick Treves, F.R.C.S. (London: H. K. Lewis. 1885.)—This is a reprint of the Hunterian Lectures delivered at the Royal College of Surgeons during the month of February of the present year, and constitutes one of the most valuable contributions to our knowledge of intestinal anatomy that has been made for many years. Mr. Treves's exhaustive monograph is founded upon the examination of no less than a hundred fresh bodies, and although, of course, it is primarily addressed to medical men, the lay zoologist and comparative anatomist will find much that is curious in it. The extraordinary variations in length and position of the various divisions of the intestines can hardly fail to strike any one who will glance even cursorily through the volume. To take the small intestine in the adult male as an example. Our author found it to vary between the extremes of 22 ft. 6 in. and 15 ft. 6 in.; while, with reference to position, we learn that the meso-colon may be expected to be found on the left side in 36 per cent. of all cases, and on the right side in 26 per cent. Abdominal surgery has made great strides within a comparatively recent period, but the grave uncertainty introduced by such variations as these must always introduce an element of danger. The work is most handsomely got up in a vellum (or imitation vellum) cover, and is printed on thick antique paper with rough edges.

Faith-healing Tested by Science and by Scripture; a Sermon. By the Rev. J. H. Skewes. (London: Geo. Philip & Son.)—As a rule, we refuse entirely to criticise

sermons; but that by Mr. Skewes shows so conclusively on scientific grounds how utterly baseless is the claim of the "Faith-healers" to any supernatural powers, that we depart from our rule to speak well of it. On his arguments from scripture we are, perforce, silent.

The Handbook of Physiognomy. By Rosa Baughan. (London: George Redway. 1885.)—That certain conformations of features are typical has been abundantly shown by the combined photographs of Mr. Francis Galton, and by those of the recent American observers. Hence any scientific attempt to classify the diverse forms of the various parts of the human face, and to show in what way any peculiar conformation may be expected to correspond with some intellectual peculiarity or gift could scarcely fail to be of value. When however we find Miss Baughan's tract bristling with crazy astrological trash about people being "born under the dominant influence of Saturn"; deriving their temperament from "the influence of Apollo or Mercury"; or exhibiting the "Signature of Jupiter," and the like, we see at once that it is about as valuable from a scientific point of view as Mother Shipton's dream-book, there or thereabouts.

School Hygiene, and Diseases Incidental to School Life. By Robert Farquharson, M.P., M.D. (London: Smith, Elder, & Co. 1885.)—It can scarcely be doubted that in a very large proportion of schools indeed hygiene, if not practically neglected, is made quite ancillary to the one object of loading the pupils up to the very muzzle with as much intellectual pabulum as can by any device be rammed into them. It is, then, alike to teach pedagogues and parents how care for the bodies of growing boys and girls is as essential as are the artifices adopted for training or forcing their minds, that Dr. Farquharson's book is written; and a real and practical work he has produced. He discourses in succession of school buildings, diet, work, and play; gives a capital chapter on the duties of the school doctor; and concludes with a tolerably full account of school diseases. The question of over-pressure in schools meets with incidental discussion, and there are some extremely sensible remarks on school-play. This is a book to be studied by all engaged in the education of children, as well as by those who perforce commit their children to others to be educated.

Key to Euclid's Elements. By John Sturgeon Mackay, M.A., F.R.S.E. (London: W. & R. Chambers. 1885.)—We are a little puzzled by Mr. Mackay's "Key," inasmuch as it contains answers to questions, solutions of problems, and demonstrations of theorems which have no existence in either of the editions of "Euclid" on our own shelves. It quite obviously has reference to some special version of the "elements" of the immortal Alexandrian; but this should have been stated on the title-page. If it be the key to a reproduction of "Euclid" edited by Mr. Mackay himself, which strikes us as being possible, such edition must be a remarkably complete one, inasmuch as a perusal of some of the very numerous answers worked out at length affords convincing proof of the pertinence of the original riders to "Euclid's" own propositions. Possessors of the particular edition of "Euclid" to which Mr. Mackay's "Key" pertains, will doubtless procure it.

Walford's Antiquarian. Edited by Edward Walford, M.A. Aug., 1885. (London: Geo. Redway.)—Full of interest to the archæologist, the historian, the herald, and the bibliographer, Mr. Walford's capital magazine may be commended to all unwilling to sever their connection with the mighty past. The continuation of a very readable article on the Bankside playhouses in Shakespeare's time will be conned with avidity by the modern playgoer who cares to contrast the comparatively rude representations of the Elizabethan Age with the elaborate scenic reproductions of the present day. A "History of Gilds" becomes important at a time when those of the City of London are threatened. Mr. Round puts Professor Freeman on his defence as a historian, while the taste of the more catholic class of readers will be gratified by the perusal of Mr. Johnson's essay on "Thackeray and His Works." We should add that we have only directly referred to four out of the twelve articles which make up the number before us.

Familiar Trees. By G. S. Boulger, F.L.S., F.G.S. With coloured plates by W. H. J. Boot. Part I. (London: Cassells & Co. 1885.)—Mr. Boulger begins his series of "Familiar Trees" with a description of that essentially typical English one, the oak; describing, within the compass of eight pages, the structure, habit of growth, uses, and parasites of the brave old tree, and mentioning certain localities in this country in which it attains great size and perfection. Life-sized coloured drawings of the oak-apple and the acorn, and a chromo-lithographic reproduction of a spirited water-colour sketch of an oak by Mr. Boot illustrate the text.

The Causes and Prevention of Blindness. By Dr. Ernst Fuchs. Translated by Dr. R. E. Dudgeon, with Notes by Dr. Roth. (London: Baillière, Tindall, & Cox. 1885.)—Of all the afflictions that can fall upon suffering humanity that of blindness is unquestionably one of the most fearful; and it is really terrible to read in the short "Report" prefixed by Dr. Roth to the volume now before us "that *two-thirds* of the 30,000 blind in England, and of the 320,000 in Europe, owe their misfortune merely to ignorance and neglect." Dr. Fuchs' book has its origin in a prize of £80, offered by the London Society for the Prevention of Blindness for the best essay in English, French, Italian, or German "On the Causes of Blindness, and the best Practical Means of Preventing it," and has been published by the Society, which awarded the well-earned prize to its author. The whole subject is treated in a manner at once lucid and attractive; and the perusal of the work will show how widely spread its interest is. A single illustration may be derived from his section treating of myopia, or short-sight, a malady so tremendously on the increase everywhere since the setting in of the educational craze; with reference to which he tells us that "myopia is the cause of 10 per cent. of all cases of blindness of one eye." Surely when concave spectacles and eye-glasses meet one at every turn, there is much to alarm us in a statement like this, given upon such authority. It may further interest a numerous class to learn that immoderate use of tobacco (and also of spirits, though the effect is less marked) causes amblyopia, or partial blindness. Full of the most practical directions for the avoidance and cure of the numerous forms of visual defects described, this is a book which should be studied by every one who values his own sight or that of his children. He that hath eyes to read, let him read it.

We have also on our table *The National Review*, a collection of thoughtful and scholarly essays, *Bradstreet's, The Sanitary News, The Medical Press and Circular, Ciel et Terre, The Journal of Botany, The Country Brewer's Gazette, Electricité, The Ilkley Free Press, The Tricyclist, Wheeling*, the inscrutable scientific journal in Arabic, *The Co-operative Index to Periodicals, Our Monthly* (Rangoon), and *Wm. Wesley & Son's Book Circular.*

The number of hands employed in Woolwich Arsenal is 11,200.

CHATS ON GEOMETRICAL MEASUREMENT.

By Richard A. Proctor.

THE SPHERE.

(*Continued from page* 120.)

A. The demonstration of the volume of the sphere seems as complete as it is simple. Of course by making the triangles *a b c*, *b c d*, in Fig. 3, &c. sufficiently small, we make the pyramid C *a b c* as nearly equal as we please to the solid sector C *a b c*. In fact, it is obvious that all the portions left over in this way will be included within a spherical shell (between the surfaces of the sphere A B D, and a concentric sphere within it touching the plane of the largest of the triangles *a b c*, *b c d*, &c.), and this sphere can be made as thin as we please by making *all* the triangles sufficiently small.

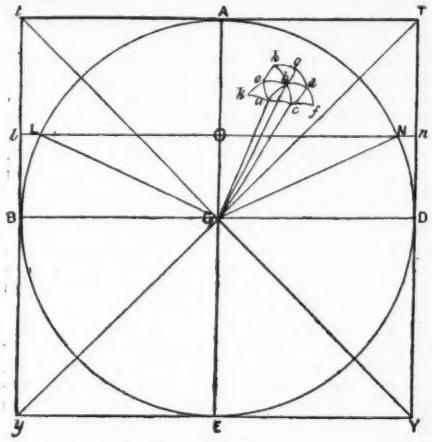


Fig. 3.

M. That seems to you obvious? I confess I think you are right; for it seems obvious to me also. Yet Euclid made this the theme of the most difficult—at least the longest—of all his problems, viz. the last problem but one of the Twelfth Book.

A. How was this?

M. Because Euclid would not allow of any construction the precise *method* of which had not been determined.

A. Can you indicate an actual demonstration, which shall not be quite so complicated as the "spider's web" proposition to which you refer?

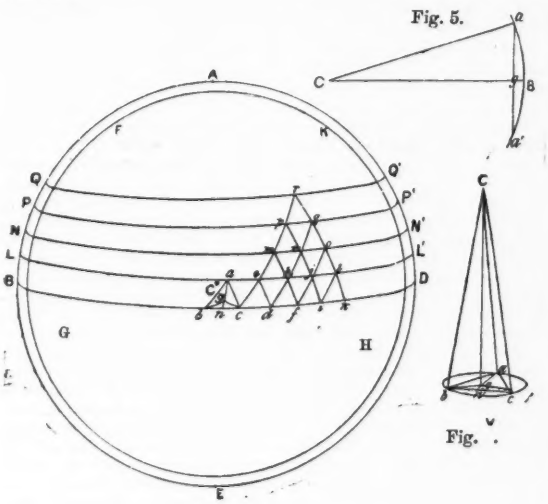


Fig. 4.

Fig. 5.

Fig.

M. I think so. Let A B E D (Fig. 4) be a sphere, of radius C B (Fig. 5), within which we wish to enclose a polyhedron which shall lie wholly without a concentric sphere F G H K, of radius C *g* (Fig. 5). Around C as centre (Fig. 5) describe the circular arc *a* B *a'*, and through *g* draw *a g a'* square to C B. Now let *a b c*, Fig. 4, be a small plane triangle having sides *a b*, *a c* equal, and *b c not greater than* either. Suppose *a b c* Fig. 6 to represent this triangle, *a b c* the circumscribing circle, *g* its centre, and *a g* Fig. 6 equal to *a g* Fig. 5. Then if *g*C be drawn in Fig. 6, square to the plane *a b c*, and equal to C*g* Fig. 5, it is obvious (since *a g* = *b g* = *c g*, and C*g* is square to each) that C *a* = C *b* = C *c* = C *a* of Fig. 5. Hence a sphere having centre C and radius C*a* will pass through *a*, *b*, and *c*; and a sphere having centre C and radius C *g* will touch the plane *a b c* at *g* (since C*g* is square to the plane *a b c*, and therefore is the shortest distance to that plane). A sphere then F G H K having centre C Fig. 4 and the same radius C *g* will *not cut* the plane *a b c*, and will only touch it if *b c* = *a b*, or *a c*; for if *b c* is less than *a b* the circumscribing circle *a b c* Fig. 4 will be less than the circle *a b c* Fig. 6, and its plane will therefore be at a distance from C exceeding C *g* (Figs. 5 and 6). *A fortiori*, any plane triangle with its angular points as *a*, *b*, *c*, on the spherical surface A B D, but having two equal sides each less than *a b*, *a c*, and the third side not exceeding either, will lie outside the inner sphere F G H K. Now it is easy to divide the whole surface of the sphere into such spherical triangles that the plane triangles having the same angular points will—as thus shown,—not touch the interior sphere. For let the great circle B D be divided into any number of equal parts, *c d*, *d f*, *f i*, *i k*, each less than *a b* (which can be done, of course, by continually halving the arcs all round, starting first with quadrants); and let the half quadrant A B be divided into any number of equal parts B L, L N, N P, &c., each less than *a n*, the arc through *a* (Fig. 4) bisecting *b c* in *n*. Let L L′, N N′, P P′, &c., be parallel small circles through L, N, P, &c., the successive points of division on the quadrant B A. On *c d*, *d f*, *f i*, *i k*, &c., as bases, let a series of isosceles spherical triangles *c e d*, *d h f*, *f j i*, *i l k*, &c., be described, having their vertices *e*, *h*, *j*, *l*, &c., on L L′; on *e h*, *h j*, *j l*, &c., another series of isosceles triangles *e m h*, *h n j*, *j o l*, &c., having their vertices *m*, *n*, *o*, &c., on N N′; on *m n*, *n o*, &c., another series of isosceles triangles *m p n*, *n q o*, &c., having their vertices *p*, *q*, &c., on P Q; and so on continually, until A is reached. It is manifest that the vertices of each circuit of triangles will draw nearer and nearer together, the farther we pass from B D; for the number of them is the same on each successive circle, and the circles continually diminish with increasing distance from B D. Thus the bases of the successive series of triangles grow less and less, as do their equal sides. Moreover the same is true, not only of the triangles as *c e d*, *d h f*, *f j i*, &c., but of the triangles, *e d h*, *h f j*, *j i l*, &c., which are also isosceles, but have their vertices turned downwards instead of upwards. Hence, doing the like with the other half, B E D, of the sphere, we have finally, the surface of the whole sphere divided into a number of isosceles spherical triangles all less than *a b c*, and none having the centre of its circumscribing circle so near to the centre as *g*. Hence the series of corresponding plane triangles form a polyhedron enclosed within the sphere A B E D, but wholly without the sphere F G H K.

A. Is that an abridgment of Euclid's proof?

M. No: Euclid's proof is different. He divides the sphere into strips, by a series of great circles all passing through the poles A, E, and each strip into spherical quadrangles by arcs of circles parallel to B D (except at the poles, where, of course, the strips end in spherical triangles). But the demonstration is very complicated, and the result does not seem to me so simple and satisfactory as when the sphere is divided into triangles.

A. You do not think, however, that any demonstration was necessary?

M. No. Take an orange—or, better, a croquet-ball—and mark dots over it so as to form a number of little triangles, beginning with a small equilateral triangle, as *a b c* in Fig. 7; and working round it with the points *d*, *e*, *f*, *g*, *h*, &c., and you will feel it to be simply obvious that you can cover over the whole surface of the sphere with acute-angled triangles (isosceles if you like, but not all equilateral) as small as you please, and therefore having the planes of the corresponding plane triangles at distances from the centre of the sphere as nearly equal to the sphere's radius as you may please. The matter as little needs proof as the fact that by adding triangles to triangles, all acute-angled, you can cover a plane surface as large as you please.

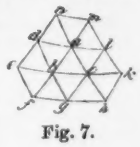


Fig. 7.

A. Why do you specially mention acute angled triangles?

M. Because the centre of a circle circumscribing an acute angled triangle lies within the triangle, and this makes the study of the problem simpler. But as the diameter of a circle circumscribed around a triangle cannot be greater than the longest side of the

triangle, (and can only be equal to that side when the triangle is right-angled) the limitation is not really necessary.

A. The problem leads of course directly to the determination of the volume of the sphere, from the known area of the sphere's surface.

M. Yes; because the polyhedron enclosed within the outer sphere A B E D, Fig. 4, differs in volume from the sphere itself, by a volume less than that of the spherical shell between the spheres A B E D and F G H K; and this shell may be made as thin as we please by making the radius of the inner sphere as nearly equal as we please to the radius of the outer.

A. Euclid, then, might have determined the solid content of the sphere.

M. Not quite. For he has nowhere shown, nor with his methods is it possible to show, either that the surface of the sphere is equal to the curved surface of the enclosing cylinder, or that the area of an enclosed polyhedron may be made to differ as little as we please from that of the circumscribing sphere. For the last point it would be necessary to show that the area of a plane triangle as *a b c* Fig. 4, may be made to differ from the area of the spherical triangle *a b c*, by a quantity which vanishes in the limit compared with either area, by making the sides of the triangle small enough compared with the radius of the sphere. No one really doubts this; no one for instance, imagines that an equilateral triangle with one-inch sides on a globe as large as our earth differs appreciably from an equilateral plane triangle with one-inch sides. But it is impossible to *prove* this.

A. What do we take next?

M. I think it now may be interesting to determine the areas and volumes of spindles.

A. I should have thought that would require the higher mathematics.

M. No; they can be dealt with geometrically, and by a method applicable to so many problems about curved areas, volumes, &c., that it is well worth studying for its own sake.

(*To be continued.*)

Miscellanea.

Obituary.—Mr. W. J. Thoms, the originator and editor up to 1872 of *Notes and Queries*, died at his residence, St. George's-square, Belgravia, on Saturday last. He was in his eighty-second year.

The necessary arrangements are being made at Chatham Dockyard for lighting the workshops and factories with the electric light, the results of the trial in those of the workshops already lighted by electricity having been found to be satisfactory.

Discovery of Mica.—A recent discovery of mica at Tallulah, Ga., is reckoned the richest in the world. It is said that blocks a foot square can be taken out, and that the supply is declared inexhaustible. There is such a demand for the mineral in stove making that it has been growing scarce and dear for several years.

The Severn Tunnel.—Considerable progress has been made with the works of the Severn Tunnel. The brickwork in the tunnel is practically finished; the western face of the tunnel is built, and the eastern face has been commenced, the ballasting is nearly completed, and the greater part of the permanent way has been laid. About 48,000 cubic yards of excavation still remain to be done in the English cutting, and about 5,000 yards on the Welsh side. Arrangements for the permanent pumping and ventilation are being practically carried out.

The Pacific Mills, situate at Lawrence, Massachusetts, are reported to be the largest textile manufacturing corporation in the world, covering forty-three acres; there are four steam-engines of 3,500 horse-power, forty-two small steam-engines, and fifty boilers and eleven turbines of 5,000 horse-power. The annual consumption of cotton is 15,000 bales; of wool, 4,000,000 lb., the product of 750,000 sheep. The annual capacity is, in cottons, printed and dyed, 65,000,000 yards; worsted goods, 35,000,000 yards, or a total of 100,000,000 yards; 3,600 females and 1,900 males are employed. The roll pay for the year ending May, 1884, amounted to 1,790,000 dols.

A Railway Journey through a Burning Forest.—During the recent hot weather a curious, but somewhat alarming, incident occurred on a railway in Finland. On approaching the town of Kaipios the driver of a train saw that the forest on both sides of the line was burning furiously, enveloping it entirely in smoke and flames. Afraid of proceeding, he despatched a messenger to the town, and after waiting for three-quarters of an hour, during which the fire had extended to both sides of the train, an engine arrived through the burning forest with the message that the line could be safely passed. Doors and windows having been well closed, the train steamed into the burning mass, and succeeded in running the gauntlet safely; but the passengers passed an anxious quarter of an hour, the heat being terrific.

Some interesting statistics in the coal trade have just been published, showing the character of the Welsh trade. The best customer Cardiff has is Port Said, which took 60,000 tons in July, and the least important amongst the principal Buenos Ayres, which took only 12,000 tons. Newport's principal customer is Genoa, which in July took 15,000 tons; Malta only 5,000 tons. Swansea's best customers were the French, Russian, and Genoese ports. During last month Cardiff sent away to foreign destinations, in round numbers, 588,000 tons of coals; Newport, 169,000 tons; and Swansea, 69,000. In the same period Cardiff sent away 13,000 tons of iron and steel; Newport, 7,000 tons of iron and steel; and Swansea, 1,000 tons. Cardiff, 18,000 tons of patent fuel; Swansea, 29,000 tons.

Timber Growth on the Pacific Coast of the United States.—The timber forests of the north-western portion of Washington Territory, U.S., contain a wealth of forest growth which is not met with elsewhere on the globe; 20,000,000 acres of forest growth are environed around the inland waterways of Paget Sound, with a coast line of 1,800 miles, indented with numerous harbours admirably suited to the manufacture of timber into merchant shapes, and loading on vessels. This timber belt, lying between the 47th and 49th parallels of latitude, will average 25,000 feet of lumber to the acre, most of which is fir, but also abounding in cedar, alder, and maple. The value of the twenty-four saw-mills in this district is estimated to be 5,300,000 dols., with a daily capacity of 1,300,000 feet, at which rate it would require 1,000 years to cut up this forest.—*Engineering.*

The Inventions Exhibition.—A supplement to the *London Gazette*, published last week, contains the jury awards—subject to revision—made in the Inventions Division of the International Exhibition at South Kensington. The awards in the Music Division will be published in October. There have been distributed 235 gold medals, 438 silver medals, 515 bronze medals, and 24 diplomas of honour. The following gold medals have been awarded by the Society of Arts on the recommendation of the Juries:—Sir Henry Bessemer, F.R.S., for the invention of Bessemer steel; Percy Gilchrist, for the Thomas-Gilchrist basic process of steel-making; Hathorn, Davey, & Co., for their domestic motor; Samson Fox, for the invention of corrugated iron flues for steam boilers; Crossley Brothers, for the "Otto" gas-engine; Ralph Tweddell, for his system of applying hydraulic power to the working of machine-tools and for the rivetting and other machines which he has invented in connection with that system; Badische Anilin and Soda Fabrik, for their improvements in the manufacture of colouring matters and intermediate products from coal-tar; William Crookes, F.R.S., for his improvements in apparatus for the production of high vacua, and for his invention of the radiometer.

The Government Astronomer of Hong Kong has published a notice with regard to typhoons, from which it appears that the earliest signs of these phenomena in the China seas are clouds of the cirrus type looking like fine hair, feathers, or small white tufts of wool travelling from east to north, a slight rise in the barometer, clear and dry but hot weather, and light winds. These are followed by a falling barometer, while the temperature rises still further. The air becomes oppressive from increasing dampness, and the sky presents a vaporous and threatening appearance. A swell in the sea, and also phosphorescence of the water, as well as glorious sunsets, are other signs useful to the mariner, who is acquainted with the usual conditions in the locality. When the typhoon is approaching, the sky becomes overcast, the temperature in consequence decreases, the dampness increases, and the barometer falls more rapidly, while the wind increases in force. Nearer the centre the wind blows so that no canvas can withstand it, and the rain pours down in torrents, but there is no thunder and lightning. Still nearer the centre there is less wind and rain, and the sky is partly clear, but the sea is tremendous. This is therefore the most dangerous position. Typhoons may be encountered in any season of the year, but are most frequent in August and September. They appear to originate south-east of the Philippine Islands. In August and September they frequently pass east of Formosa, or travel towards north-west up through the Formosa Channel, or strike the coast of China. Afterwards they usually recurve towards north-east and pass over Japan or across the sea north of Japan, but not with the violence that is characteristic of tropical storms. During the remainder of the year they most frequently cross the China Sea from east to west.

XUM

Our Inventors' Column.

SHIPS' SIGNAL LAMPS.

[Patent No. 15,147. 1884.]—This invention, by Mr. Edwin Martin, of 68, West India Dock-road, London, E., is intended as a means of ensuring better light and absolute freedom from being extinguished by the violence of the weather, irrespective of its severity. It is, therefore, entitled to greater consideration for the prevention of collisions at sea, which are too frequently caused through the lights becoming extinguished.

It has two lenses, an outer and an inner, either of which may be coloured; between the two lenses there is a space, about 1½ in. wide, for the purpose of admitting a current of air between them, the object being to keep them cool.

The inner lens is fitted inside the body of the lamp, and a shield-plate is so fixed to the lens and sides of the lamp as to prevent any up or down draught coming into contact with the light.

The shield-plate, which is the chief novelty, is fixed to back and sides of the lamp. It is bent conical, and is left open at the top end under the raised top of the lamp.

The outer lens is fitted to a hinged frame, for the more ready convenience of keeping it clean and preventing an accumulation of dirt whereby the full advantage of the light may become unobtainable.

At the bottom of the lamp is a row of air-holes, so constructed as to maintain a clear current of air, and allow it to pass away either top or bottom.

The lamp is proof against being extinguished either by wind or water, without regard to its application.

Colza or petroleum oil may be used for burning, and the method for trimming is similar to that adopted in the ordinary signal lamps now in use.

The body or frame of lamp may be made any size or material.

Trials of Mr. Martin's Lamp have been made with the top open.

1st.—A blast generated by a fan driven by steam power was played on it from every direction; this trial lasted about ten minutes.

2nd.—The lamp was run through a trough of water ten feet long, being held under water.

3rd.—Water from a fire-hose was played on the lamp from all directions for about ten minutes, the top being left open. None of these trials produced any effect.

The lamp had been lit about half-an-hour before the trial took place, so that the metal and glass had plenty of time to heat. It must be admitted that by a very simple and inexpensive contrivance, the patentee has accomplished what he claims for it—viz:—

It cannot be blown out by the wind or put out by water, and is so constructed that the lenses will always keep cool.

These lamps appear to be exceptionally well suited for large or small craft, such as coasters and fishing-vessels that usually carry their lights near the water, as they are not liable to be blown or jerked out by the quick motion of such craft; neither would the lens be damaged or the light affected by sprays or seas going over them.

SLOW FOCUSSING MICROSCOPE ADJUSTMENT.

[Patent No. 12,952. 1884.]—This invention of Messrs. Swift & Sons, of 81, Tottenham-court-road, W., consists of a dovetail slide placed parallel to the optical-tube of the instrument; it is lifted by a trigger-shaped lever, and is depressed by means of a spring from above. The lever is moved by a fine thread-milled head-screw, one turn of which moves the optical-tube a 1,000th part of an inch, thus rendering it sufficiently delicate for all kinds of high-power microscopical investigation, and, by lengthening the lever from its fulcrum, any degree of slow motion can be produced. The dovetail slide and the entire mechanism that moves it is in both directions parallel to the line of motion, thus completely obviating the displacement of the object by tilting—a very common defect in all other fine adjustments where the motive power is not central to the said line. As none of the mechanism enters the optical-tube, this is left quite free for the adaptation of the analysing and binocular prisms, which can be brought much closer to the posterior lens of the objective than is usually the case, thus permitting the use of higher powers than when the fine adjustment is fitted within the microscope tube. One great advantage in this form of fine adjustment over the many others is that the distance between the eyepiece and objective is maintained when this movement is in action. Although it is so exceedingly delicate in use, it will resist the strain of adapting either two or four objectives in their double or quadruple piece.

"Let knowledge grow from more to more."—Alfred Tennyson.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

All Editorial communications should be addressed to the Editor of Knowledge; *all Business communications to the* Publishers, *at the Office, 74, Great Queen-street, W.C.* If this is not attended to, delays arise for which the Editor is not responsible.

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METEORIC DISPLAY.

[1874]—I should be very glad to hear through Knowledge that other readers noticed the meteoric shower that occurred on Monday evening between 9.15 and 10.40. At the same time a brilliant display of lightning illuminated the sky on the eastern horizon.

All the meteors appeared to diverge from a point just below Cassiopeia in the Milky Way. The brightest fell from the zenith in a S.E. direction, about 9.26. When near the horizon it burst, the particles being of a very intense blue colour and formed a tail like a comet's, which was visible for fifteen seconds.

Several others were seen, but none so bright as the one described.

At 10.40 the sky became overcast with clouds.

Bracebridge, Aug. 10, 1885. Jas. Grant.

[The display belonged to the well-known August shower—the Perseids.—R. P.]

METEOR SHOWERS.

[1875]—I should feel much obliged if you would clear up some difficulties in my mind on the subject of meteor showers.

Some years ago I read with the greatest interest a lecture delivered at the Royal Institution, on Feb. 14, 1879, by Mr. G. J. Stoney, F.R.S., on the "Story of the November Meteors." Your articles on meteors in this year's Knowledge considerably discount the value of this otherwise most interesting lecture. To account for these meteors which once travelled together, afterwards lengthening into a procession, he conjectures that the November shower was once (in A.D. 126 probably) a cluster close to Uranus, when the individual meteors, being perturbed in greater or less degree, tended to separate, those most attracted lagging behind. Furthermore, he says at p. 9—"As the comet (with a cluster of meteors within it) swept past the planet, its outlying parts would seem to have grazed his surface, and in this way the gas was probably somewhat more retarded than the meteors; and in the centuries which have since elapsed the meteors have gone so much ahead of the comet that they are now treading on his heels, and on the point of overtaking him, while, probably, the gas has again brought together a smaller cluster of the meteors."

In one of your articles you show clearly that this reasoning is quite fallacious, and that, paradoxical as it might seem, the perturbation would have quite a different effect.

My difficulty is mainly this, that while your argument appears quite correct, Mr. Stoney's would seem to accord better with the evidence.

Here are the points I wish cleared up:—1. Is it not a fact that meteor-trains do lengthen out in the lapse of ages, early in their history being condensed in a compact body like the Leonids of November, and finally straggling so much as to be scattered all round their orbits as the Perseids of August?

2. If they do lengthen out, does it not agree better with Schiaparelli's or Stoney's theory than with yours, which, I take it, assigns to each meteor a new orbit, with major axes lying in different directions, the point of perturbation being the only point common to each orbit?

3. The "gem of the ring" of the Leonids is now a long way from the point of the earth's orbit traversed on Nov. 14.

Would any stray meteors of this shower be seen next November?

XUM

If so, how would they get away so far from the main body? (I may mention that last year I watched for them after midnight, but as I saw none in half an hour, I went to bed.)

4. Is the following a valid objection to your theory of a meteor's—or rather a meteor—train, being ejected from planets, sun, or stars? Observation or analogy show that all these bodies rotate. If a shower takes two days or two years to fly past a given point, we must presume (unless a lengthening-out happens in transit) that the shower took two days or two years for the process. But if the process did last so long as this, it would be impossible for all the ejected matter to be expelled from the rotating body in the same direction—unless, indeed, the material was discharged from the poles of the rotating body.

The fixed radiant-point question is very interesting, and I hope a good exposition of the subject will appear in KNOWLEDGE.

MORE LIGHT.

[1. Meteor systems lengthen out. 2. This agrees with my theory at least as well as with Schiaparelli's. 3. The "gem of the ring" of Leonids has passed completely away from the neighbourhood of the earth's orbit. We may, however, be shortly expecting to see some of the meteors scattered in *front* of the main body. 4. If a shower takes two days or two years *now* in passing a given point, it by no means follows that the original cluster took that time in starting—on the contrary we know the present extension of the flight is greater than of old. Hence this peculiarity implies no objection at all to my theory.

Lastly I may remark that my theory was suggested years after I had examined and described the peculiarities "More Light" touches on, besides a number of others which few of those who just now treat of meteors seem to remember—assuming they ever knew them. My essays, written in the years 1866–71, on the November meteors alone would make a good-sized book.—R. P.]

INVISIBLE SUNS.

[1876]—I was much interested by an article by Mr. Proctor which appeared in KNOWLEDGE a few months ago, pointing out the comparatively small number of orbs which are likely to be inhabited at any one time. It seems to me that the same chain of reasoning leads to the conclusion that the universe must contain many invisible suns—great orbs like our sun, which have cooled down sufficiently to emit no light of their own.

Some of these may probably be in a condition, as regards temperature, permitting of their habitation by living creatures. It is a curious speculation, What forms of life would be found on such a globe, illuminated, as it would probably be, only by starlight?

MUSAFIR.

SUN WORSHIP.

[1877]—If the writer of the letter No. 1835, page 79, in KNOWLEDGE for July 24 of this year will give in KNOWLEDGE the source of his information respecting the sun worshippers of Mexico and Peru who "held dogmas almost identical with Christianity," I shall be extremely obliged.

ZETEO.

LIFE IN THE MOON—DEATH OF PLANETS—BLUE SKIES—MOISTURE OF THE ATMOSPHERE—SOLID OXYGEN—THE WATERS ABOVE THE FIRMAMENT.

[1878]—By referring again to letter 1812, "Hallyards" will find that the remarks contained therein, concerning imaginary men in the moon, are just as applicable to his new example of Uley Bury as they were to the case of the Pyramids, which he has seen fit to lay aside.

Unless "Hallyards" can demonstrate the probability he assumes, the age of life in the moon must have overlapped a period, the exact counterpart of a given period of life on the earth, and yet not have exceeded the present; he may throw back the age of man and his work as many centuries as he pleases without affecting the question in the least.

In the absence of an occasional fact or even probability to guide, it is penetrating far enough into realms of imagination, to picture either an inhabited moon, or a moon never the abode of life, without defining a limit to the civilisation or brain power of the merely-conjectured Lunarians. Furthermore, the wish to compare specimens of human work, lunar and terrestrial, without a comparison of the circumstances giving rise to the work in either case, shows a decided tendency to build "castles in the air."

I must inform "Hallyards" that I do not "contend" for the "man in the moon," my *contention* is simply that there may have been men in the moon for all he has said to disprove it. There are perhaps more weighty reasons against an inhabited moon than those advanced by "Hallyards." (By-the-way, I should like to hear a few particulars of "Uley Bury" and H.'s authority for its remote age.)

"There is nothing to show that planets die, so long as they have their sun." Which *is* (or rather *was*) the sun of the moon, the earth, or our sun? The earth, I should think; and since the moon's sun has cooled down sufficient to bear life, the *death* of its planet can be easily understood. What will the earth be like when *our* sun is cool enough to bear life?

The "positive fact" that northern skies are bluer than those of the "sunny south" needs some slight corroboration.

Walking through a picture-gallery a day or two ago, I could not help noticing how different tints of blue (independent of cloud colour) were characteristic of different latitudes. We are all prone to mistakes ("H." not excepted); but seldom is the fact so exemplified, as in the case of our artists in their endeavour to reproduce nature, if "Hallyards" in this instance be correct.

Once again, "Hallyards" has supplied me with the argument against himself. A better proof that the amount of aqueous vapour must be greater in warm climates than in cold, I could not have wished than that instanced by the sudden introduction of "French rolls" into this discussion. Because in France, "rolls" and things in general are unusually dry, he concludes that the atmosphere likewise is dry. Will "Hallyards" explain where the moisture of the rolls, &c., has gone? It has simply evaporated, which means, the solvent power of air being raised by an increase of temperature, the water required to saturate it is drawn from wherever water is exposed, be it lake, river, rill, or "roll."

I am asked to "demonstrate that (the atmosphere of) the parched Soudan has more water than the storm-swept Arcades." If the foregoing example is not sufficient, the following figures will explain:—

Air absorbs one hundred and sixtieth part of its weight in aqueous vapour at 32°, one-eightieth at 59°, one-fortieth at 86°, summer heat, and a twentieth at 113°, intense heat.

As to solidified oxygen and nitrogen, like the discovery of the conjectural metal hydrogenium, it has been accomplished numberless times within the last twenty years, if every scientific announcement of the fact is to be credited—we have been deceived so often that incredulity is somewhat pardonable. I made the assertion that these gases could not be solidified on the authority of at least half-a-dozen standard *Chemistries*, but I will not dispute the point, as it is of no moment. I admitted in last letter that all gases might be solidified.

Referring to the "waters-above-the-earth" theory, a scanty glance through the first chapters of Genesis will show "Hallyards" that its origin is shrouded in no ambiguity, and we are not even dependent upon the Hebrew records for its exposition; it was an universal idea.

ALEX. MACKIE.

[The question of the possibilities of past life in the moon though suitable enough for suggestions, is not one for discussion. We can fancy many things but can establish nothing.—There can be no doubt that there is more moisture usually in the air over hot regions than over cold ones. Oxygen and nitrogen have both been solidified,—and by more than one process.—R. P.]

EVOLUTIONISM.

[1879]—So much has been lately written in KNOWLEDGE about the Darwinian theory, that perhaps I also may be allowed to raise a point or two that seem as yet untouched.

I cannot help thinking that the reason why Darwinism is so generally accepted at present is because it is a theory so admirably suited for our present phase of thought. In these days of inventions, of rapid advance in science and in material civilisation, when the ideas of all instinctively turn, whether they will or not, in the direction of the unlimited development of human resources, the notion even of an inexpansible millennium becomes too narrow for us, and in such an age this new evangel of progress could not fail to have an immense following. A century ago, had there been found a historian who could philosophise on history as we do now, and could he have foreseen the enormous strides that have lately been made in science and its application to material resources, he might have been able to predict that a theory of this nature would arise—he might even have laid down some of its main characteristics. The moral to be drawn is this: the more completely a theory is the creature of its age, the more deeply ought it to be distrusted.

Again, it really seems to me that the "struggle for existence" has too much to carry. It appears to be forgotten that slight variations (the only kind admitted) are *absolutely and totally* ineffective in this struggle because their results are so small. To illustrate: I wear moustaches on my lip which in eating continually get inconveniently into my mouth, but are most ungraceful

when clipped short; nor will I take the trouble to shave. Is it, then, to be supposed that in the struggle for existence the men whose moustaches incline more outwards will have so much the best of it that, after many hundred—or, if you will have it, let us say, million—generations, all mankind will be found to have their moustache hair pointing towards the outer edge of their eyes?

Then, again, animals have the power of adapting themselves to circumstances. Everybody will, I suppose, admit that. It manifests itself in many ways, as when a cat takes on a thicker fur in cold countries, &c. In such cases there is no time for direct evolution to come into play; such adaptation sometimes takes place in the life of a single animal. Now this power itself, say the Darwinians, is a product of natural selection, since the possession of such a power is evidently favourable to the species. But when we come to look closer it becomes plain that this power in animals is in itself quite sufficient to account for the whole process of development. It is quite as effective for that purpose as the theory of natural selection could be. Darwinianism, therefore, instead of being the great panacea for the explanation of everything and everybody, is in reality an explanation of nothing but this one power. But by such a limitation Darwinianism loses its *raison d'être*, for the only justification of a hypothesis is its power of co-ordinating a great number of varied phenomena.

P. J. Beveridge.

[Mr. Beveridge's letter contains ample internal evidence that his knowledge of the theory of evolution is derived from hearsay, and that his personal acquaintance with it is of the very haziest and most imperfect character.—Ed.]

[When one considers Darwin's unwearying zeal and industry in collecting and observing facts, his marvellous patience and skill in analysing them, and his freedom from prejudice in interpreting them, such facile and flippant comments as are passed on his work by some who manifestly have never read with attention ten pages of his master works, are amusing—to speak mildly of them.—R. P.]

WHAT LIES OUTSIDE THE MECHANISM OF THE UNIVERSE?

[1880]—Mr. Proctor, in his clever article entitled "George Eliot on Mental Decay," has sufficiently exposed the false reasoning due to theological bias; but I think it will be found that in enforcing his argument he has given undue prominence to mechanical doctrines, and has incurred the risk of being misunderstood upon a very important subject. It is very natural to overstate a truth when you have antagonists who can only dogmatise and assert; and if I am right in my assumption, I, for one, can sincerely sympathise with the able Conductor of Knowledge in this respect. But I presume (and I hope I am right) that Mr. Proctor believes in something else than pure mechanism. It would not be unscientific for him to suppose with Herbert Spencer that there is an unknowable and infinite energy underlying phenomena. [Why this is the very religion of science, the soul of the worship of science, the foundation of the whole system of duty inculcated by science. Where is the man of science who does *not* believe this?—R. P.] There is nothing theological in the conception, and it would be more in harmony with facts than causeless mechanism. Supposing this to be the view of Mr. Proctor, it does not appear in his arguments; and I call attention to the circumstance that he may correct any misconception, if there is such.

In supporting George Eliot's views by illustrations of his own, drawn from music, he makes B. say:—

"And yet, after all, there are musical passages, whose beauty seems independent of the material qualities of the instrument."

To which A. answers:—"Not one. . ."

C. then asks:—"So music depends on mechanism after all?"

A. replies, and through A., Mr. Proctor:—"Undoubtedly. It depends absolutely on mechanism."

Now, I contend that this is not quite in harmony with the facts. If mere beauty of sound were concerned the statement would be perfectly true, but if musical thought and conception is referred to as a part of music—which it is unquestionably—then it is not true at all, or only to a small extent. Beethoven was deaf the greater part of his musical life, and had to write without hearing an instrument. Of course, he mentally heard them, but instruments did not create the music which came unbidden to his mind. *His* was the motive power, as was also that of Mozart and others, which created the mechanism of music. They left the art where they did not find it, they created a necessity for more perfect appliances. The modulations which they introduced into music made the equal temperament necessary, and the combinations of tones they conceived altered and fashioned many physical features in the art.

I may have misunderstood Mr. Proctor, but it seems to me that he has unduly emphasized the mechanical element in nature. Certainly, one school of "materialists" lay great stress upon this feature; but the other school—to whom, I think, Herbert Spencer belongs—while admitting final causes to be unknowable, yet admit in that unknowable element the potency and creative energy for all we see.

Gamma.

[Although I think it unnecessary to explain that I meant what I clearly said, I insert "Gamma's" letter, as presenting well certain points which are often misunderstood. I said music depends absolutely on mechanism, not music *is* mechanism. The same mechanism may be used to bring out the music of a Bach a Beethoven a Handel or a Mozart, the melodies gay or sad which move the musically untaught, the wearisome iteration of the common sorts of dance music, or the meaningless thumping of a child. Every one of these products depends on the mechanism. That they are so unlike shows that there is something outside the mere mechanism. As my object was specially to show that George Eliot's words had not the meaning which "Gamma" thinks may be found in mine,—and as I definitely showed this by the illustration I derived from music (note especially the distinction dwelt upon in the last paragraph but two), "Gamma's" elucidation was unnecessary. But being good, in itself, let it stand.—R. P.]

"DOUBLING UP YOUR HAVES."

[1881]—I have a great respect for Mr. Proctor's opinions on most subjects, but I must confess that I entirely agree with "Hallyards" in considering "I should have like to have seen" an odious phrase, and I am surprised to find Mr. Proctor defending it. And yet similar phrases are constantly used, not only in common parlance, but in books otherwise well written. Most persons will confess it is incorrect when their attention is called to it, but a few maintain that all the verbs in a sentence should be in the same tense. Mark Twain calls it "Doubling up your haves," but then at present he is not regarded as an authority by Knowledge. A similar expression is, "I meant to *have gone*," when what was originally "meant" was "to go."

E. C. H.

[I did not defend the phrase, and I never use it. What I said was that it conveys a certain meaning not quite conveyed by any other form of expression. Mark Twain is not an authority here on science; but a writer of so much experience is necessarily an authority on the use of words and forms of expression.—R. P.]

"THE WELL OF ENGLISH UNDEFILED."

[1882]—Much has been said of late respecting the loose and inaccurate way of speaking and writing common among people who presumably are fairly educated, and I may perhaps be allowed to call attention to the following vulgar illiteratisms:—"Different to," for different *from*. "Those sort of people," for *that* sort. "Either side of the way," when the sense clearly indicates that *each* should be used; an error very common among popular authors. "I shall have much pleasure in accepting," &c., instead of I *have*, *cum multis aliis*, but the above are among the most general errors of common parlance. With regard to pronunciation, one may often suppose that the rule in our dictionaries that the comma after and above a syllable indicates that it is accentuated is not generally understood, or we should not so often hear such mistakes as, centrifu'gal, contripe'tal, Ser'apis; and among the clergy, inspi'ration is far more common than the *i* in the second syllable pronounced like *e* short.

These are a few indications of a faulty system of education in the past, which in large schools especially apparently took for granted that children were intuitively gifted with a fair knowledge of their own language, or were grounded as infants.

H. A. Bulley.

P.S.—I heard the present Bishop of London called pedantic for rightly pronouncing i'solate, is'olate.

[Prof. Young, of Princeton, N.J., assured me that "different to" is a specially English mistake. It is such a bad mistake that I can hardly think it as common in English writing as he says he finds it. "Differing with" a person is another bad mistake. "Averse to" is not so bad, in fact custom has not yet established "averse from,"—"averse" may be equivalent to "adverse," and so the "to" justified. And certainly, "I am averse from litigation" is less supported by custom than "I am averse to litigation." "Either" for "each" is admissible in poetic descriptions, whether in metre or otherwise. "On either side there lay a gulf profound," "on either side stood Death in awful form,"—here certainly "each" is really meant, but "each" would be utterly prosaic. "I shall have much pleasure" &c. though not strictly correct, is an idiomatic way of conveying the idea that the result of accepting the imitation *now*, will be pleasurable *hereafter*. I have never heard centrifúgal or centripétal.]

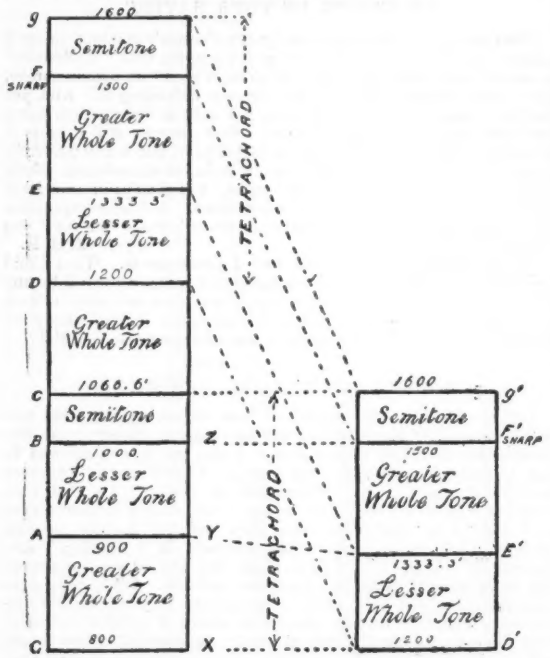
XUM

DE GUSTIBUS—MUSICAL TEMPERAMENT.

[1883]—When the classification which I put forward in letter 1742 is taken into account—namely of (I.) physical excitement, (II.) intellectual satisfaction, and (III.) associations, &c.—as three elements in questions of taste, I think many quotations from critics in painting might be made in justification. The early Florentine and Roman schools will seem to have more of the spiritual, the Venetian somewhat more of the sensual element, and the Flemish a great deal more of the sensual. But at present I have to offer an illustration from another art, where, for want of observing the above distinctions, a controversy remains open, in which mathematically-exact philosophers appear arranged on the sensual side of the question, as opponents to practical professors, who take the intellectual side; a curious position of things.

My present subject is Musical Temperament; which I propose to take on the way—a roundabout way as it may seem—towards resuming the consideration of Symmetry. (I must still use the word loosely for convenience, notwithstanding the objections of "Another Old Draughtsman"—letter 1797—which will deserve regard at another time.)

The subject may perhaps be new to some of your readers, and I proceed to give an elementary statement.



If the distances G X or A Y represent a second of time, and if in that time a sounding body, associated with the line G X, make 800 audible vibrations, while at A Y are made 900, and at B Z 1,000 vibrations, the three effects are such as are known respectively as the keynote of a musical scale, the so-called second, and the so-called third of that scale.*

In these cases, with low common measures, such as 8, 9, and 10, the notes have readily-apprehended relative individualities; and, moreover, imaginary distances, as A G or B A, become readily conceivable to the musical faculty, like distances extended between the lines of an inscription or between stars in the sky. The distances between the lines G X, A Y, or A Y, B Z, are called in musical language intervals of major seconds, or whole tones, by a confusion of a word for a sound with one for an interval between sounds.

The acoustic philosopher discriminates further, and distinguishes the greater tone A G from the lesser tone B A. Perhaps he plumes himself on superior accuracy, because the amount of the interval is according to the ratio of numbers of vibrations, and 8 : 9 is greater than 9 : 10.

* The key of G has been chosen, because at the high pitch of modern orchestras the note G above the treble stave would be very close to 800 vibrations per second. If I had selected 80, 90, and 100 vibrations they would have constituted deep bass notes; 8, 9, and 10 per second would not have been musical at all.

The foregoing numbers illustrate "Just Intonation." Recourse might be had to tuning A Y, so that it should consist of 895 vibrations instead of 900. In that case as (barring fractions) 800 : 895 :: 865 : 1000; the two intervals G A, A B will be equal. This will give an idea of what is termed Equal Temperament in opposition to Just Intonation.

In practice, however, something else must be done, for B Z itself must be tempered; and we must have B Z at 1008 vibrations, and A Y at 898; in which case 800 : 898::898 : 1008. With which proportions musicians seem generally content in the present day, in the case of keyed instruments.

But sweetness of tone is associated with low, common measures of the ratios of vibrations. The "3rd" so called, of 800 and 1000 vibrations, in proportion 4 : 5, is especially sweet; more so than 800 : 1008; whose lowest terms are as high as 50 : 63.

How do these things concern the hearer? The sweetness is of sense, sensual. The individuality of 1008 vibrations in opposition to 800, results in an interval, only by a trifle less intelligible to the understanding than the very sweet interval.

If the intellectual is to give way to the sensual, we want a diatonic octave scale with these intervals, viz., keynote to 2nd, a greater tone, with the ratio 8 : 9 vibrations; 2nd to 3rd, a lesser tone of 9 : 10 vibrations; 3rd to 4th, a semitone of 15 : 16 vibrations; then a greater tone followed by a lesser, then a greater, and then another semitone. These constitute a justly-intoned scale (G g). But the system of music (not acoustics) implies liberty to a composer to lead hearers, to draw intellectual analogies between certain sets of notes called tetrachords.* A tetrachord consists of two larger intervals (tones) followed by a smaller (semitone). Upon the equal temperament system these analogies may be minutely true. Upon the system of just intonation they are loose analogies. Thus, let the notes of one tetrachord justly intoned be put on a level with another, dismounted from the upper part of the same octave—G C on a level with D′ g′—and let cross-lines be drawn between the corresponding parts, G X D′, A Y E′, &c. It will then be seen that the line A Y E is crooked. The justly-intoned tetrachords are not symmetrical.

But if the tetrachords had been equally tempered, the line A Y E′ would have been straight, and the "canonical" imitation of a musical phrase in one group of notes G C by another group D′ g would have been a close one. Thus the intellectual understanding of music is served as well by equal as by just intonation, however the unsophisticated outer sense of hearing in a few persons of extraordinary delicate sensation may be offended by it.

The third element, of association, may unite sometimes with the intellectual branch, sometimes with the sensual. And if the association be with the past sensations of childhood, or again be a lingering relic of ancestral sensations, there seems less coarseness than the word "sensual" generally implies. Time purifies it.

A. O. D.

IS THE GAME OF DRAUGHTS PLAYED OUT?

[1884]—I was much amused by a sentence in letter 1856. "The game of draughts is now played out." On referring to the *Encyclopædia Britannica* I find that the writer of the article "Draughts" practically says the same. He says that the game is exhausted, and that the best reply to every possible move is known. I wish to call your attention to a few facts which will show the falsity of this statement. There are in England and Scotland about twenty newspapers which contain a Draught column. Week by week these columns publish *new* play. There is also a weekly magazine devoted entirely to draughts, and it contains weekly a large amount of original play. Recently a pamphlet was published containing sixteen pages of new and original play on an hitherto unexplored opening.

In April, 1884, an international match was played between England and Scotland. Both countries were represented by the finest players to be obtained. The result was—England, 7 won games; Scotland, 36 won games. Would this have been so if all the best moves had been known? Not one-hundredth part of the possibilities of draughts are yet known. All the best players are agreed that draughts is yet in its infancy.

The writer of the article in the *Ency. Br.* cannot have had any personal knowledge of the game. He is unknown to players whose memory can carry them back twenty or thirty years.

A. E. Hodgson.

SMELL-CLOUDS.

[1885]—We see the vapour of water travelling in well-defined masses, and sometimes are plunged into them. Hence we do not dispute their consistency and locomotion. We do not see other

* Let any who doubts, deliberately consider the definition of a "fugue," and particularly a so-called "real fugue," in music.

masses of gas, and for this reason we do not realise to what an extent such lumps of rare matter hold together and travel.

In 1882 I addressed to *K.* a speculation as follows. After a violent westerly gale I *sometimes* remark here for days after an entirely novel odour like primeval forests. I have conferred with natives, and, as usual, they give an explanation which cannot be the true one—*i.e.* that it comes from kelp-burning at Noirmantier, 10 miles off. I point out that we almost always have the wind just from that quarter—S.W.—*without* any smell—(2) that when we have this new perfume, it is after a *W.* gale of exceptional force—(3) that the smell is not kelp, but pines, and general verdure.

My suggestion to *K.* was that this smell had come all the way from America. If caught in the centre of a cyclone, I see no reason why it should not pass the Atlantic, kept entire by its very rotation. I instanced two remarkable cases of cohesion of odorous gas. At noon on a calm sunny winter's day I passed four or five young ladies seated on a stone bench. I noticed as I passed an abundance of strong perfume. They would not stay there long; an hour, at most. Yet, when I repassed after dark, I found the perfume quite as strong about the bench. On the same spot a few years before I passed and repassed several times through an invisible cloud of wood-smoke, deliciously odorous. This must have come from a good distance, for there were no inhabited houses near.

My letter was not noticed in any way. It no doubt seemed mere extravagance.

In June 1884 a friend lent me a N.Y. or Detroit paper (which I regret to have lost) wherein it was reported that a ship's crew had been almost suffocated by meeting, 400 miles from the nearest land, a dense bank of wood-smoke. If smoke will travel 500 miles, why not 2,000 for a backwoods bouquet?

Last night on going out I felt myself in the poisonous fumes of a lime-kiln. There is one a mile to windward. In half-an-hour there was no more smell. The cloud of gas had travelled on.

I have stood on Killiney Hill on a windy day, and seen Dublin quite free of smoke, while over Howth and Sutton there hung a black pall, which was the smoke of Dublin, detached. I have smelt the smoke of Dublin twenty miles off, and have been told that sea-foam has been found in the centre of Ireland. At Brompton after long-prevailing E. wind I have found the air not better than that of Fleet-street. This might turn the scale in favour of death, in the case of a very delicate sufferer; but I have always found physicians calmly supercilious about any such obscure causes of variation in strength. HALLYARDS.

SOMETHING ABOUT THE SKIN-CASTING OF SNAKES.

[1886]—During a service out here of thirty odd years, I have come across snakes, I may say, by the thousand; and I have met them in the most unlikely localities, and most extraordinary positions.

Some of the latter they could not possibly have reached without using their ventral scales to assist them in climbing—*e.g.*, a mud wall; or, as the pedal-scales of lizards to occasion a vacuum, and enable the individual to ascend the interior of a glass jar.

The most extraordinary locality was finding a poisonous snake coiled up between the seat and lid of a dak-bungalow night-stool; and the next, the double roof of my brougham; and dropping a cobra from the sleeve of my wife's velvet jacket, which had been hanging on a dress-stand.*

And the localities in which I have found the exuviæ of snakes have been equally singular. Sometimes I have found them amongst the entanglements of a rose-bush, and entire, too. The krait, a small and very poisonous species, is a snake of literary attainments, and affects book-cases, and I have found its skin on the top of a shelf of books; I have found a cobra-skin left as a card on the floor of my drawing-room; I have met it stretched out on the broken brick-ballast of the railway.

Apropos to this, it was an old tradition out here that snakes abhorred rough places, and, therefore, our houses used to be surrounded by broken stones or bricks. I soon saw, from meeting snakes in these so-called protected houses, that this was a delusion, and that animals which have no legs, wings, or arms, can be met with as freely up a tree as on a house-top; and that they can progress as easily along the face of a mud wall as on the ground.

But I am wandering from my subject—the exuviæ of snakes, or skin-casting.

The books say that because the scale and corneæ plates are always found inverted, this wonderful process of exuviation must commence at the tail and end with the head.

To explain the book process I must resort to a rough diagram. Having shrunk within its skin, and thus loosened it, that around the jaws cracks, and the snake begins to wriggle forwards, the tail adhesion being as yet perfect; half-way through the process the book position is shown roughly in Fig. 1 (the dots indicating the skin), and it can be imitated by passing a needle with a knotted

Fig. 1.

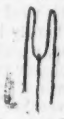


Fig. 2.

thread inwards through the tip of the finger of an inflated glove, and then pulling gently on the thread, as shown in Fig. 2. The process of inversion goes on gradually, and ends by the tail passing through the skin of the mouth; it is then detached and the exuvium is left behind. But how this extraordinary detachment takes place, the books don't say. As the result of this process the mouth of the skin is left exactly opposite to the point of exit of the snake—*i.e.*, if the snake began exuviating with its face to the East, the mouth of the skin will point to the west.

All this is too complicated for the real simplicity of Nature's work; exuviation is rendered necessary by the snake having outgrown its skin, which is therefore dispensed with, and the process is exactly similar to that seen, say, among silkworms, only that in their case the exuvium is left behind crumpled up, and not straightened out. I have never seen anguine exuviation, but, judging from that of caterpillars, the loose skin is slowly thrown off by a series of undulatory movements.

Charmers, out here, carry about their snakes, cobras generally, and sometimes pythons, in circular baskets, and when skin-changing comes on (at the commencement of the cold weather), the poor creatures are in great straits, and can only relieve themselves by rubbing off flakes of epidermis against the sides of the basket. In the natural process, the inversion of the corneæ and scale-plates, is, I believe, due partly to cuticular contraction in drying, and partly to atmospheric pressure. R. F. HUTCHINSON, M.D.

Pachmari, July 5, 1885.

* [My own father-in-law, during the time his regiment was stationed in India, found a "tic-polonga," a most deadly snake, in his flute!—ED.]

LETTERS RECEIVED AND SHORT ANSWERS.

AGRICOLA. Not the very slightest. It is stuff and nonsense from beginning to end.—REV. LACY H. RUMSEY. Thanks, but more than one such table is already in existence. *Vide* (for example) pp. 886 to 891 of Chambers's "Descriptive Astronomy," De Morgan's "Book of Almanacs," &c., &c. Kindly note, too, the paragraph, in capital letters, which concludes those heading the Correspondence Columns.—AUGUSTUS J. HARVEY opines that there are not enough clubs, societies, associations, and institutions in London and other cities and towns in the kingdom; but, in view of the collapse of so many of such establishments, I fear that he will get but few people to agree with him.—JOHN HAMPDEN. Do you seriously expect for a single instant that any *gentleman* can, or will, take the slightest notice of such unmitigated ruffianism? Mr. Wallace can only possibly treat it with that supremely contemptuous silence which it deserves. [So much from my friend the acting editor. Mr. Hampden's letter has been forwarded to me, and I may as well give my opinion also. Mr. Hampden writes to me that he has called on a gentleman whom he describes as a friend of mine (but whom I have never met), that he tried to insult that gentleman most grossly, and that failing in this, simply because Mr. Hampden's violence was beneath a gentleman's notice, he distributed over eighty offensive handbills in the neighbourhood. Mr. Hampden seems to implore me to notice what he is capable of in his own special line. He asks my opinion how a gentleman treated as he has treated one of the most esteemed men of science of the day might be expected to act. My opinion on this point is that this gentleman displays marvellous self-possession and dignity. The *probabilities* I should imagine were largely in favour of a sound horse-whipping being the reward of the conduct Mr. H. describes, assuming his account correct.—R. P.]—W. WOOLSTON MOODIE. The mathematics essential for the reduction of astronomical observations are plane and spherical trigonometry, the use of logarithms, and interpolation by differences, a competent knowledge of all which is a *sine quâ non*. It would be advantageous, too, to master the method of least squares, for obtaining the most probable value of a large number of varying observations.—DR. LEWINS. I have not inserted your more

recent letters, because they simply consist of the reiteration of the previous exposition of your thesis admitted into these columns. Judging from the correspondence (or rather utter absence of it) elicited by your letters and those of Miss Naden on the same subject, the seed fell upon but barren ground indeed. For some reason which I will make no attempt to fathom, the readers of KNOWLEDGE do not appear to care two straws about Hylo-Idealism.—ALEX. INGLIS. I do not know whether such forecasts as the late Lieut. Saxby used to publish are now sold. I imagine that if sold they can hardly be "got for a payment" small enough to represent their value, which would be somewhat less than nothing (at least if the purchaser put faith in them).—W. C. PENN. Questions can *not* be answered by post. Yes; great faith is put in the moon as an index of weather by country people; also in fortune-telling, and other foolish fancies.—J. BAILEY. Certainly, *I* should understand upwards of one hundred to mean more than one hundred, not less. But never mind *me*.—W. N. RIDLINGTON. Editor has nothing to do with such matters. Try local newsvendors.—F. W. H. AND METER. From opposite sides you deal with the question of necessity and supreme law. To one I answer that if necessity makes a man objectionable, it becomes necessary that he should be corrected. We may, as you seem to suggest, feel sorry for him, because of his necessity; so we might feel sorry at having to shoot a tiger who had been driven by necessity to propose eating us. To "Meter" I note that his or her view, the ordinary theological view, is open to some trifling objections. If only one view of right and wrong had commended itself to the nations, the idea that that view was of supernatural origin, or divinely inspired, would run to some degree on all-fours. But as this is very far from being the case, as multitudinous ideas about right and wrong prevail in different nations, and are nearly all thought to be of divine origin, a certain degree of difficulty is introduced. However, though Mr. Foster was allowed to examine here the system of ethics arising, as Mr. Herbert Spencer has shown, out of the process of evolution, and commending itself as best for the progress and welfare of the world, he carefully refrained (you may have noticed) from discussing the question of a divinely-communicated moral law. The subject is one which cannot be dealt with in these columns.—G. W. DODDS. Do not know. Surely the makers or sellers would give you the best advice on such points.—R.F.H. I see no great difficulty about the unfortunate condition of the Dumba Sheep. If that creature had by ordinary evolution, acting simply through natural selection, reached that unsatisfactory state, of course it would be strange to consider how a race had actually been brought to its end by a process which usually fits races better to contend with adverse conditions in their surroundings. But with domestic animals, artificially selected and treated, such peculiarities can readily be understood. The Sandwich Islanders are a case in point; for their race would come to an end, if my information is correct (and it comes from their king), but for special surgical assistance.—W. H. BULPITT. Thanks.—J. H. M. The reviewer's innocent remark must not lead to a discussion about the innocents. The subject is outside our range, and few, I fancy, care much about it.—F. WEST. Have forwarded your article to the writer who deals with inventions.—R. A. H. We shall not have space, I fear, for the papers you kindly suggest. They are a little outside our line. (With regard to a personal interview, your chambers, though near the publishing office of KNOWLEDGE, are not within fifty miles of the room where I am writing these replies.—A. BROTHERS. The angle subtended by the image does not change at all. Surely the result observed requires no explanation. What else *could* happen?—S. B. B. The puzzle is well known. All the cut squares, though they seem to fit so as to form other squares, really form incomplete figures.—COMMENTATOR. Your "Darwinism debated" is more than debateable.—W. REYNOLDS. Letter should have been sent to "Mephisto." Your corrections are right; but surely no one could be for a moment in doubt.—G. G. G. Neither do I (R. P.) know the author of those lines. The thought is tender and touching, the form occasionally rough,—as in the line "And lovedst all and renderedst good for ill." Does not the *popular conception* of a future life, so viewed — as all men who have loved and lost must view it, if they accept it—seem unutterably sad? Even the mere poetic presentation of that which my reason rejects is for me inexpressibly saddening. Thanks very much for the stanzas.—R. I. P. "*Has* Hallyards ever been a certain successful novelist."—ALEX. MACKIE. A slight change.—ROBERT LEWINS, M.D. A story occurs to me (R. P.) apropos of your theory of Hylo-Idealism, which I may throw in here in reply to your latest communication—received since the Acting Editor replied to you as above. A man in "the States" was commenting unfavourably on the Royal arms of England and their heraldic supporters. He objected definitely to the Crown, and effusively abused the Lion,—but his remarks on the Unicorn were comparatively vague:—"*Gawldarn* a *Unicorn*, any way" was his sentiment in regard to that highly idealistic creature. I think the general interest in Hylo-Idealism is equal in enthusiasm to that felt in the Unicorn of Heraldry—there or thereabouts.—The letter on M. Renan and G. Eliot trenches on dangerous ground.—P. M. YEARSLEY. Quite agree with you. But oh! don't let us invite that one to express his views. One who "never seems at a loss on any subject" is seldom to be trusted.—ONE WHO IS NOT CONNECTED WITH THE VEGETARIAN SOCIETY, THE STUDY OF SUN-SPOTTERY, THE ROYAL ASTRONOMICAL SOCIETY, OR A CERTAIN WELL-KNOWN WEEKLY SCIENTIFIC PAPER—what a *nom-de-plume!*—is bewildering. As he sends anonymous impertinence, he probably *is* connected with the Vegetarian Society, &c., &c. But he may be merely crazed. Who is "*the* astronomer who is going to regenerate the world by harping on with his ideas about 'sun-spottery'?" "One who, &c.," implies that the Conductor of KNOWLEDGE harps on that way. But KNOWLEDGE has had very little to say about sun-spottery, and that little has not come from the Conductor. In former years, I grant "One who, &c.," the Conductor of KNOWLEDGE exposed the trick by which some impecunious persons tried to get money and position out of ingeniously-imagined influences of sun-spots on our weather: but the snake was not only scotched, it was killed at that time; long before KNOWLEDGE was started. Who hopes to get money out of that dead trick *now?* What is there, by the way, about sun-spots in KNOWLEDGE for August 7th, that "One who &c." points to that number? Does he in any way or degree know what he is talking about? I imagine not. From the rudeness of "One who &c." to the contributors to KNOWLEDGE, I imagine he has wished to contribute and been found—naturally—unequal to the work. His writing—though disguised—seems familiar to the conductorial eye.—W. PATON. I (R. P.) am most emphatically of your opinion. Within a few weeks you will have good evidence on that point.—J. E. WALKER. It is most improbable that the indistinct articulation is caused by the tubes. Possibly the carbons of the distant microphone have "set," or the fault may be that the diaphragm in the home receiver is out of adjustment. The best plan would be to ask the lessors to overhaul the apparatus.

NOTICE.—In future weekly numbers of KNOWLEDGE, the answers to correspondents will occupy much reduced space. We can no longer undertake to answer all questions addressed to us, or to explain why letters intended to be published are not suitable for our columns.

Our Whist Column.

BY "FIVE OF CLUBS."

SOLUTION OF PROBLEM.

I FOUND when I reached England (for it is of course now no secret that "Five of Clubs" and the "Conductor of KNOWLEDGE" are one and the same person) that I had not sent, as I supposed, the solution of the fine double-dummy problem which appeared on June 26. I had written a full analysis of the problem, and had certainly intended either to send it with the problem, or with the next batch of papers forwarded from my home in Missouri. But it is clear I did not do so, and that analysis is lying somewhere *perdu* among the multitudinous papers, letters, pamphlets, maps, &c., &c., &c., which I have brought to my home in England. This is the more unfortunate as I find very few of our Whist-readers have succeeded in solving the problem. Of course many, knowing that it originally appeared,—as I have recently learned—in the *Field*, would not have had anything to say about it, having already either solved it or seen the solution. But from the letters of many it appears to have been freely tried, and with very little success. Some who have been thus foiled have objurgated me very freely for the omission to give the promised solution, one or two deeming the delay merely a clever trick on my part either to cover some mistake in the problem or to increase the number of readers of KNOWLEDGE. I should have thought the latter effect would certainly not be produced by any trick of the imagined sort, but quite the reverse; while as for delay being a satisfactory way of trying to hide a mistake, I have observed that the longer correction is deferred the worse are the effects of a mistake, and my own constant rule has been to acknowledge any mistake I may have made as quickly as possible.

However, there is no mistake about this particular problem. Here are the hands and conditions:—

Spades—7, 6.
Hearts—Kn, 4.
Clubs—A, 3.

Hearts—Q, 9.
Clubs—Kn, 8.
Diamonds—Kn, 6.

Hearts—10, 5, 3.
Clubs—6.
Diamonds—Q, 7.

Hearts—A, K, 6.
Clubs—10.
Diamonds—10, 9.

A leads, and *A-B* are to make all the tricks, let *Y-Z* play as they may. (A correspondent remarks that I did not originally mention that Spades are trumps, though that was obvious from my comments; but it has been our constant rule to indicate the trump suit by the use of italics, and the Spades were thus marked, clearly enough for all the Whist readers of KNOWLEDGE, as trumps.)

Singularly enough this problem which seems so difficult, yields at once when dealt with—not as a puzzle but—as a position in actual play. Following correct Whist principles the solution of the problem is simply the play which a good double-dummy player in *A*'s position would certainly follow. I was nearly a quarter of an hour working unsuccessfully at this problem as a puzzle; but so soon as I tried the experiment, which I have occasionally found successful with Mr. Lewis's masterly double-dummy problems—of playing according to the best perception of the position, I solved the problem at the first attempt,—(first on that line, *bien entendu*).

Observe: *A* has five certain tricks from the two trumps, the Ace, King of Hearts and the Ace of Diamonds; he may get a sixth trick either in Hearts if *Z* can be made to discard a Heart, or if *Z* can be led through in hearts after first trick in that suit, or in Clubs if *Y* can be made to discard a club, or in Diamonds if both *Y* and *Z* can be made to discard their last Diamond after one round trumped by *B*. To keep open these chances, *A-B* must retain the power of leading through *Z* in Hearts after one trick has been taken in the suit, and this lead *must* be with *B*'s small heart or *A* can never make his tenace over *Z* except on sufference; while also, obviously, the longer *A B* keep back the lead from their two card plain suits the better chance there will be of forcing discards from *Y-Z* in these suits. Clearly then the most promising play for the first trick is as follows:—

A	*Y*	*B*	*Z*
HK	H9	HKn	H3

No Whist player of any experience would hesitate for a moment about the play of the Heart Knave, seeing that *B*'s retention of the card would obviously leave *Z* free, when *B* eventually led the Knave, to play his lowest; when, if *A* captured the Knave, the third trick in Hearts would go to *Z*; while if *A* failed to capture it, *B* would have no Heart to lead and must lead a losing Club. Nor can we doubt that it is best for *Y* to play the Heart Nine. On one possible line it is true *Y-Z* might get a trick through *Y* playing the Queen; but only by *A-B* playing incorrectly.

One of his five certain tricks has now been taken by *A*. Of course he must not now go on with Hearts. Equally obvious is it that he must not lead his Club; for that is a card necessarily valueless in *A*'s hand, and it may become a matter of importance to be able to discard this absolutely valueless Club to *B*'s lead of a trump. Everything in such cases depends on holding open as long as possible the lines of play available.

Therefore the second lead *must* be the Diamond Ten, to which *Y* and *Z* each drop their smallest Diamond, which *B* trumps with the Six.

It is now *B*'s turn to lead. He clearly must not lead the Club Ace *here*, for the same reason that *A* should not lead Club at the preceding trick. Equally clear is it that he must not lead a Heart; for *A* would be forced to play his Ace *Z* retaining his Ten, and securing one trick for *Y-Z* whatever *A* might lead.

Therefore the third lead *must* be *B*'s long trump. Clearly *Z* can lose nothing by discarding his single Club; *A* can lose nothing by discarding his. But what shall *Y* discard? If he discards his Heart Queen, *A*'s tenace over *Z* is made safe, *B* simply leads his Club Ace, making the fourth trick, and then a small Heart, *A* making the two remaining tricks in Hearts. If *Y* discards a Club, *B* makes the fourth and fifth tricks with his Club Ace and Three, the sixth going to *A*'s Heart Ace. It remains only for *Y* to discard his Diamond Knave.

A-B have now made three tricks; but even yet *Y-Z* seems to have command over all three plain suits. They have between them,—

Second and third best Hearts with the fifth best,
Second and third best Clubs,
Best Diamond,

moreover the second and third best Clubs are in one hand, and the third and fifth best Hearts in one hand. For all this *A-B* have the game in their hands, and their play is now quite simple, since *B* obviously must not lead a Heart.

The fourth lead *must* be *B*'s Club Ace. *Z* must either discard a Heart or his Diamond Queen. If he discards a Heart, *A* discards his Diamond Nine, and *B* leading the small Heart *A*'s Ace and Six make the remaining tricks. If *Z* discards his Diamond Queen, *A* discards his Heart Six, and on *B* leading a Heart the remaining tricks fall to *A*'s Heart Ace and Diamond Nine. *Y* of course follows suit with Club Eight.

The full solution runs then as follows:—

	A.	*Y.*	*B.*	*Z.*
1.	H K	H 9	H Kn	H 3
2.	D 10	D 6	S 6	D 7
3.	C 10	D Q	S 7	C 6
4.	H 6	C 8	C A	D Q
5.	H A	H Q	H 4	H 5
6.	D 9	C Kn	C 3	H 10
or 4.	D 9	C 8	C A	H 5
5.	H A	H Q	H 4	H 10
6.	H 6	C Kn	C 3	D Q

The student will find it a useful exercise to study out why it is that though Club Ace, and Spade 7 are both winning cards, it makes all the difference between winning and losing whether *B* at the third trick leads one or the other. The reason will be found to be that if *B* leads the Club Ace first, *A* at the fourth round has to decide on his discard before knowing *Y*'s play: but if the Ace leads the fourth round A knows already what must be *Y*'s play.

ANSWERS TO CORRESPONDENTS.

F. H. LEWIS.—Many thanks. The words in parentheses would have been better omitted. I was dealing, however, at the outset with a purely imaginary case. I was thinking of an exposure by accident, to third player *only*,—as for instance of early in the game fourth player had held his cards so carelessly that third could not help seeing the Ace.

J. OSBORN.—There is no misprint in the Double Dummy problem given in Clay. Don't give it up: but if you must you will find the solution in "How to Play Whist" (Longmans).

AN INDIFFERENT WHIST PLAYER, CONSTANT READER, P. C. B., J. R. L., EMU, L. K., SCATTERBRAIN, and others. Solution and explanation of delay now given.

Solutions of Double Dummy problem by PETERKIN, S. JONES, MARLBOROUGH, K. J., and TWO OF CLUBS all correct; all others wrong. The six tricks cannot be made if *B* does not play the Knave first round—that is, if *Y* and *Z* play properly. Nor if *B* leads Club Ace third round.

J. L. P.—Not very wonderful that I missed a problem in the *Field* for January 31, considering that between December 1884 and March 1885 I was on a lecture tour which carried me from the extreme north-east point of the United States, through New York, Washington, and the Southern States, to Texas. The *Field* does not reach one in many of the places I then visited.

W. J. WALLACE.—The difference between the frontispiece of "How to Play Whist" and the description of the problem in the body of the book is absolutely without importance—the cards interchanged between *Y* and *B* being of equal value so far as trick-making is concerned.

HOME WHIST, a short and simple introduction to the game of Whist, with all the latest developments, including the American Leads, and the Echo in Plain suits, by "Five of Clubs" (Richard A. Proctor), is now in type and will shortly be published by Messrs. Longmans. It is illustrated by eight games showing the play of four sets of hands, first in the old-fashioned style of Home Whist, and secondly according to true Whist principles. These will serve as an introduction to the forty games illustrating "How to Play Whist," by the same author (Longmans).

XUM

Our Chess Column.

By Mephisto.

ILLUSTRATIVE GAME No. 7.

CONTESTED in the Hereford Tournament of the Counties Chess Association, August 12, between Herr Schallopp, of Berlin, and Mr. J. H. Blackburne. A remarkably well-played game, one of the best in the tournament. It is an excellent illustration of the advantage to be gained by vigorous play, against defective development in the Opening. It also contains an attack against the French Defence, which, to the best of our recollection, we have never before seen attempted in practical play.

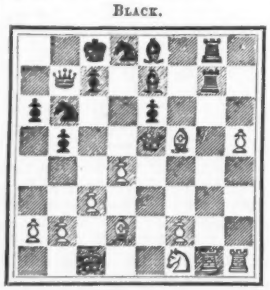
White. Schallopp.	Black. Blackburne.
1. P to K4	P to K3
2. P to Q4	P to Q4
3. Kt to QB3	Kt to KB3
4. B to Q3 (*a*)	P × P (*b*)
5. Kt × P	Kt to B3
6. Kt to KB3	B to K2
7. P to B3	B to Q2 (*c*)
8. B to B4	P to KR3
9. P to KR4!	P to QR3 (*d*)
10. Q to K2	Kt to Q4
11. B to Q2	P to QKt4
12. P to KKt4! (*e*)	Kt to Kt3
13. Castles QR	Q to B sq. (*f*)
14. P to Kt5!	P to KR4
15. P to Kt6 (*g*)	

Black.

White.

	P to B3
16. Kt to Kt3 (*h*)	Q to Kt2
17. Kt to R2	Castles QR
18. Kt × P	QR to Kt sq.
19. Kt × P! (*i*)	R × Kt
20. P to R5	P to B4
21. QR to Kt sq. (*j*)	Kt to Q sq.
22. Kt to B sq.	B to K sq.
23. Q to K5	R (Kt2) Kt sq.
24. P to Kt7!	R to R7
25. B × BP!	KR × P

Black.



White.

26. R × R	Q × R (*k*)
27. R × R	Q × Kt (ch)
28. K to B2	B to Q2
29. P to R6 (*l*)	Kt to B5
30. Q to K4	K to Kt sq.
31. P to R7	P to Kt5
32. Q to K sq. (*m*)	Kt to R6 (ch) (*n*)
33. P × Kt	Q to B5
34. P to R8 (Q) (*o*)	

Notes.

(*a*) Every move that developes a piece, without giving the opponent a good chance of doing the same, is a distinct gain of time. Formerly White in this Opening played at an early stage P × QP. This liberated Black's QB, and gave him quite as much freedom of action as White had. 4. B to KKt5 has mostly been played of late; but, except that it occasionally enables White to surprise Black on the K's side if he is injudicious enough to Castle too early, nothing much comes of that move.

(*b*) We cannot approve of this capture. In the first instance, White establishes a piece on K4, from whence it can only be effectually displaced by P to KB4 on the part of Black, which move, however, would leave his KP weak. Then again, by playing P × P Black secures White's position, and removes a weakness in White's game, which ought to help Black in his development. For instance, by leaving the P en prise, and playing B to Kt5, Black at once threatens to win a P. He therefore compels White to do something, and thus gains time for development. If in reply to 4. B to Kt5 White plays 5. B to KKt5, the position is that of the game between Gunsberg and Mason at the Hamburg Tournament, in which the latter continued with 5. P to KR3. 6. B × Kt, Q × B. 7. P to K5, Q to K2. 8. Q to Kt4, Q to Kt4, &c.

(*c*) Had Black Castled, Herr Schallopp intended to proceed with 8. Kt × Kt, B × Kt. 9. P to KKt4, instituting an attack on the K's side.

(*d*) Not knowing how to act, Black intends to seek an opening on the Q's side in a very round-about way. All things considered, it would have been the lesser evil for Black to have chosen the following line of play:—9. Kt to Q4. 10. B to Q2, P to B4. 11. Kt to Kt3, B to B3, &c.

(*e*) White's attack is both energetic and original, the game throughout does not follow the conventional lines which by most players are too anxiously adhered to in match play.

(*f*) Black is actually driven to this resource for bringing his Q out of the way, to enable him to Castle.

(*g*) Every move of White is played with telling effect. If Black takes the proffered P, White would further improve his game by Kt (K4) to Kt5.

(*h*) Going at once for the isolated RP.

(*i*) It is quite refreshing to see such a splendid example of originality and enterprise in attack. White, not content to win by the P he has gained, gives up the piece to obtain two passed P's.

(*j*) Herr Schallopp thinks that KR to Kt sq. would have been better, for subsequently Black moved his Kt, and brought the Q to bear on the KR, thereby retarding White's advance.

(*k*) The game is full of interest at every step. If Black had played R × R, White could proceed with 27. B to K4, followed by P to R6 or Q × R. In any case White has several ways of winning.

(*l*) Threatening mate.

(*m*) Leaving no square for Black to retire his Q.

(*n*) A desperate effort to open White's game with a view to a perpetual check.

(*o*) The game extended another dozen moves or so, after which Blackburne had to resign, his ingenious efforts having been of no avail.

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